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No. VIII.—AUGUST, 1895.

AN UNCROWNED KING IN SCIENCE.

IN MEMORIAM.

THOMAS HENRY HUXLEY, P.C., D.C.L. (OXON),

LL.D. (CANTAB, EDIN., ET DUBL.), M.D. (WÜRZB.), PH.D. (BRESLAU), F.R.C.S., F.R.S., F.L.S., F.G.S., F.Z.S., TRUSTEE BRIT. MUS., ETC.

BORN MAY 4TH, 1825. DIED JUNE 29TH, 1895.

(With a Portrait.)

By the death of Professor Huxley another illustrious name must now be inscribed on the walls of our Valhalla, while the world of science mourns the loss of one of its most distinguished leaders, who shares with Owen and Darwin the reputation of having done more to advance the study of biology than any other of the naturalists who have appeared within the present century.

Though he won his grandest triumphs as a zoologist, there were not many departments of Natural History which he did not cultivate; indeed, it has been truly said of him, by Haeckel, that he was one of the few investigators who had thoroughly mastered the whole range of biology, and might claim to be the first zoologist in this country. Although capable of specializing in any group of animals or plants, he never lost sight of the broader biological problems which are so often overlooked by the less broad-minded systematist, so that he was able to enter the more limited field of classification, and give those who devoted their whole time to any one group a lesson on their own subject. He may well be regarded as our greatest naturalist.

Thomas Henry Huxley was born at Ealing on May 4th, 1825, and was for some years educated at the school in his native place, where his father was one of the masters. This preparatory course was followed by assiduous private reading, including German scientific literature, and instruction in medicine received from a brother-in-law who was a physician. He afterwards attended lectures at the Medical School of the Charing Cross Hospital.

In 1845 he passed the first examination for the degree of M.B. at the University of London, taking honours in physiology. In 1846 he was appointed Assistant-Surgeon to H.M.S. "Victory," for service at Haslar Hospital, and seven months later he was gazetted as

Assistant-Surgeon to H.M.S. "Rattlesnake," under the command of Captain Owen Stanley (brother of the late Dean Stanley), commissioned to survey the intricate passage within the Barrier Reef skirting the eastern shores of Australia, and to explore the sea lying between the northern end of that reef and New Guinea. It was the best apprenticeship to what was eventually to be the work of Huxley's life, the solution of biological problems and the indication of their far-reaching significance.

Two of his warmest friends, Darwin and Hooker, had passed through a like curriculum, the former as Naturalist to the "Beagle" on her voyage round the world in 1831, and the latter as Assistant-Surgeon on board the "Erebus" on her Antarctic expedition in 1839. Eventually the three stood shoulder to shoulder when the battle against the immutability of species was fought.

The voyage lasted from 1847 to 1850, and was the initiation of Huxley's scientific career. Some of the results of the studies in Natural History, for which the cruise afforded facilities, were transmitted to the Linnean and Royal Societies, and were in due course published in their Transactions.

Returning to England in 1850, Mr. Huxley was, in the following year, elected a Fellow of the Royal Society, and in 1852 he was presented with one of the Royal Medals annually awarded by the Society. In 1854 he received the appointment of Professor of Natural History, including Palæontology, in the Royal School of Mines and Curator of the fossil Collections in the Museum of Geology, Jermyn Street; and in the same year that of Fullerian Professor of Physiology and Comparative Anatomy to the University of London.

In 1856 he accompanied his friend Professor Tyndall in his first visit to the glaciers of the Alps, and with him read a joint paper on Glacial Phenomena, published in the Philosophical Transactions in 1857. In 1858 he was appointed Croonian Lecturer to the Royal Society, when he took for his subject "The Theory of the Vertebrate Skull." In 1859 his monograph on "The Ocean Hydrozoa, a description of the Calycophoridæ and Physophoridæ observed during the voyage of H.M.S. 'Rattlesnake,'" was published by the Ray Society. In 1860 Prof. Huxley delivered a course of lectures to working-men, in Jermyn Street, on "The Relation of Man to the Lower Animals." The questions arising out of this topic became the subject of warm controversy at the meeting of the British Association at Oxford between Bishop Wilberforce and Professor Huxley, and was taken up by others in that and subsequent years. The whole discussion appeared in the work entitled "Evidence of Man's place in Nature" (1863), and excited great popular interest both in this country and abroad. Mr. Darwin's views on the origin of species formed Professor Huxley's subject for his lectures to working-men in 1862, subsequently published under the title, "On our knowledge of the Causes of the Phenomena of Organic Nature." His other lectures were on the "Elements of Comparative Anatomy" and on the "Classification of Animals and the Vertebrate Skull."

In 1862, while serving as one of the Secretaries of the Geological Society, he was called upon, in the absence of Mr. Leonard Horner, the President, to deliver the Annual Address. Again, in 1869 and 1870, whilst filling the Presidential Chair of that Society, he delivered two other addresses. That in 1862 dealt with the subject of *Homotaxis* as opposed to the use of the term *synchronism*; in 1869 with the limitation of geological time; in 1870 with the evolution of the vertebrata and their geographical distribution. He presided over Section D at the meeting of the British Association at Cambridge (1862), and as President of the Association at Liverpool in 1870, delivering appropriate addresses on both occasions.

From 1863–70 he held the office of Hunterian Professor of Comparative Anatomy in the Royal College of Surgeons, and in 1869–70 was President of the Ethnological Society. He filled the office of Secretary to the Royal Society for ten years, 1871–80, and of President from 1883 to 1885. In 1883 he was appointed Rede Lecturer at Cambridge.

During the absence of Prof. Wyville Thomson on the "Challenger" Expedition, he divided with Dr. Carus, of Leipzig, the duty of acting as his substitute, delivering the lectures in the summer sessions of 1875 and 1876 at the University of Edinburgh. In 1876 Prof. Huxley visited America, when he delivered an Address at the opening of the Johns Hopkins University at Baltimore, on September 12th, and three lectures in New York, on September 18, 20, and 22, before crowded audiences. In the same year Huxley received the Wollaston Medal from the Geological Society of London.

He was elected a member of the London School Board in 1870, and took a leading part in opposing denominational teaching; but was compelled by ill-health to retire from the Board in January, 1872. In December, 1872, he was elected Lord Rector of Aberdeen University for three years, and installed February 27th, 1874. He was a Fellow and Governor for some years of Eton College. He was an elected Trustee of the British Museum, and a member of the Senate of the University of London. He served on many Government and Royal Commissions, notably on Science, on Fisheries, on Contagious Diseases, on Vivisection, on the Scottish Universities, etc. From 1881 to 1885 he held the office of Inspector of Salmon Fisheries. The only post he continued to hold up to the time of his death was that of Dean and Honorary Professor of Biology in the Royal College of Science, South Kensington.

In 1892 he was admitted a member of the Privy Council, having previously refused the honour of knighthood.

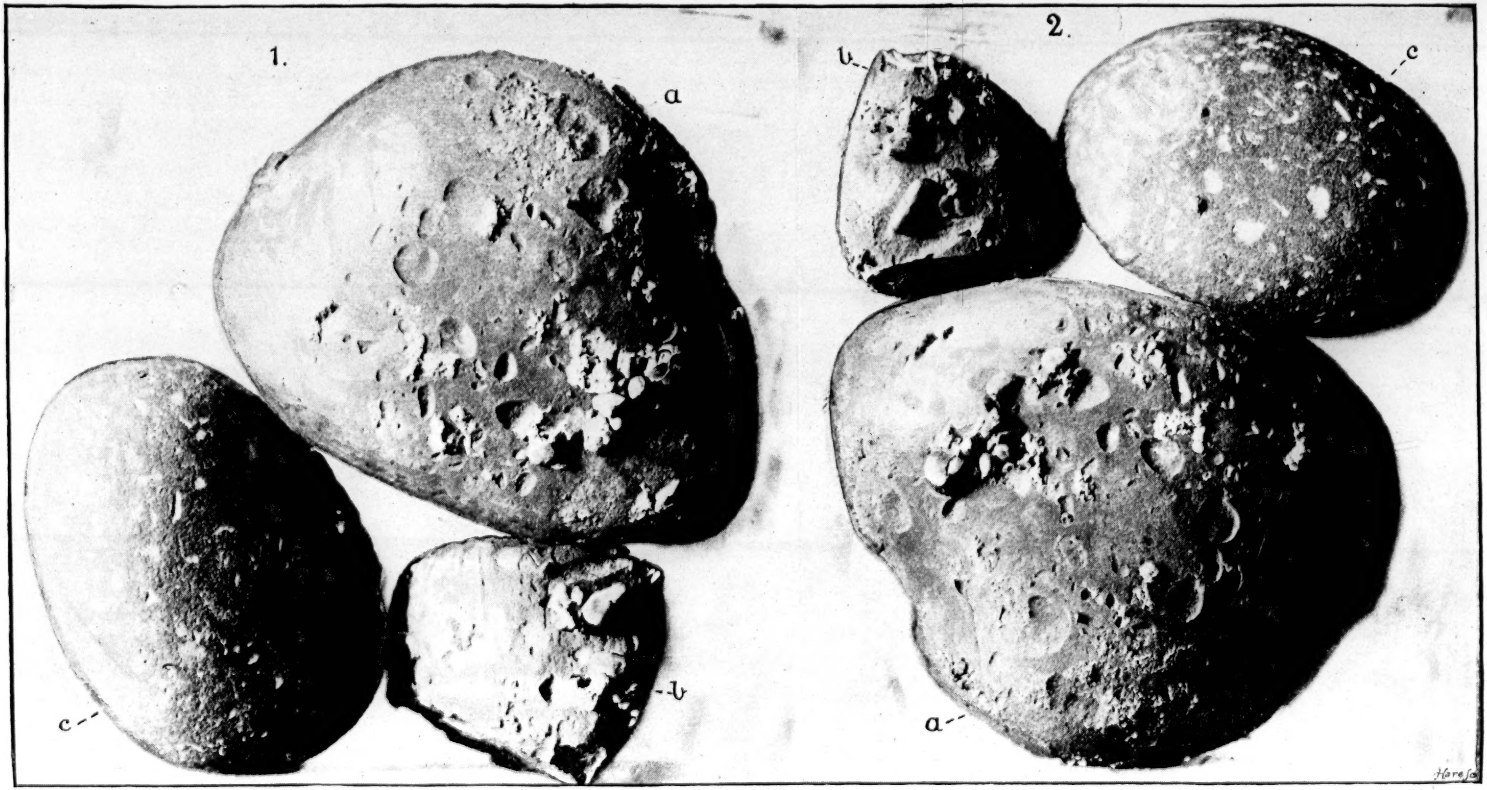
It is impossible to enumerate here the many honours conferred upon Professor Huxley. He was made a Doctor of the Universities of Edinburgh, Dublin, Cambridge, Oxford, Breslau, and Würzburg. The Academies of Brussels, Stockholm, Copenhagen, Cairo, Berlin, Göttingen, Haarlem, St. Petersburg, Lisbon, Rome, Munich, Philadelphia, and many others, conferred on him their Diplomas. He was made an Honorary Fellow of the Royal Society of Edinburgh;

a Member of the Royal Irish Academy; of the American Academy of Science; and (in 1879) a Corresponding Member of the Institute of France (Section Anatomy and Zoology, in place of Von Baer). He was also a Riddare of the Pole Star of Sweden.

Turning to his published works, we may refer to his "Oceanic Hydrozoa": his Lectures on Comparative Anatomy and Physiology; Lessons in Elementary Physiology (1866), and many subsequent editions; An Introduction to the Classification of Animals (1869); "Lay Sermons, Addresses, and Reviews" (1870). His textbooks on the Anatomy (I) of the Vertebrata (1871), and (II) of the Invertebrata; his Practical Biology; "Man's Place in Nature"; on the Cray-fish, and on Physiography, well illustrate the wide extent and versatility of his powers, both as a naturalist and author; but it was by his lectures and addresses that he displayed the most marvellous of his intellectual gifts, and produced the greatest effect upon the science of his time. He had that wonderful power of carrying his audience along with him, and the happy facility of bringing his knowledge within the mental grasp of his hearers.

Of the 144 papers attributed to Prof. Huxley in the Royal Society's list of scientific papers extending from 1847 to 1884, the following may be mentioned as directly connected with our own science:—

On the Method of Palæontology (Annals, 1856). *Pygocephalus Cooperi*, a Coal-measure Crustacean (Q.J.G.S. 1857). On the genus *Pteraspis* (Brit. Assoc. Rep. 1858); On *Cephalaspis* and *Pteraspis* (Q.J.G.S. 1858); On *Plesiosaurus Etheridgei* (Q.J.G.S. 1858). On Persistent Types of Animal Life (Roy. Inst. Proc. 1858-62). On Species and Races and their Origin (Roy. Inst. Proc. 1860). On *Stagonolepis Robertsoni* (Q.J.G.S. 1859); On some Amphibian and Reptilian Remains from South Africa and Australia (Q.J.G.S. 1859); On *Dicynodon Murrayi*, South Africa, and on Skulls of Dicynodonts (Q.J.G.S. 1859); On *Rhamphorhynchus Bucklandi*, a Pterosaurian from Stonesfield (Q.J.G.S. 1859); On a Fossil Bird and a Fossil Cetacean from New Zealand (Q.J.G.S. 1859); On Dermal Armour of *Crocodilus Hastingsiæ* (Q.J.G.S. 1859); On the Anatomy of *Pterygotus* (Geol. Surv. Mem. 1859); On *Dasyceps Bucklandi* (Geol. Surv. Mem. 1859); On the Lower Jaw of a Labyrinthodont (Geol. Surv. Mem. 1859). On *Macrauchenia Boliviensis* (Q.J.G.S. 1861); On *Pteraspis Dunensis* (Q.J.G.S. 1861); Systematic Arrangement of Devonian Fishes (Geol. Surv. Mem. 1861). New Labyrinthodonts from Edinburgh Coal-field (Q.J.G.S. 1862); On a Stalk-eyed Crustacean from Coal-measures, Paisley (Q.J.G.S. 1862); On the Premolar Teeth of *Diprotodon* (Q.J.G.S. 1862). On a New Species of *Glyptodon* (Roy. Soc. Proc. 1862-63). *Anthracosaurus Russelli*, Coal-field Lanark (Q.J.G.S. 1863). On Cetacean Fossils termed "Ziphius," Cuvier, from the Red Crag (Q.J.G.S. 1864). Osteology of *Glyptodon* (Phil. Trans. 1865). Vertebrate Remains from Jarrow Colliery, Kilkenny, Ireland (Geol. Mag. 1866). Dinosaurian Reptiles from South Africa (Q.J.G.S. 1867); On *Acanthopholis horridus*, a new Reptile from the Chalk Marl (Geol. Mag. 1867); New specimen of *Telerpeton Elginense* (Q.J.G.S. 1867). Animals intermediate between Birds and Reptiles (Geol. Mag. 1868); Two new Fossil Lacertilians from South Africa (Geol. Mag. 1868, pp. 201-205); On *Archæopteryx lithographica* (Roy. Soc. Proc. 1868). On *Hyperodapedon* (Q.J.G.S. 1869); On a new Labyrinthodont, *Pholiderpeton scutigerum*, from Bradford (Q.J.G.S. 1869); On the Upper Jaw of *Megalosaurus* (Q.J.G.S. 1869); Principles and Methods of Palæontology (Smithsonian Reports, 1869). The Milk-dentition of *Palæotherium magnum* (Geol. Mag. 1870); On *Hypsilophodon Foxii*, a new Dinosaurian from the Wealden, Isle of Wight (Q.J.G.S. 1870); Further evidence of the Affinity between the Dinosaurian Reptiles and Birds (Q.J.G.S. 1870); On the Classification of the Dinosauria, with Observations on the Dinosauria of the Trias (Q.J.G.S. 1870); Triassic Dinosauria (*Nature*, 1870, p. 23); On the Maxillæ of *Megalosaurus* (Phil. Mag. 1870). (With Dr. E. P. Wright) On the Fossil Vertebrata from the Jarrow Colliery,



Pitted Pebbles from the Bunter Conglomerate, Cannock Chase.

Kilkenny, Ireland (Irish Acad. Trans. 1871). On *Stagonolepis Robertsoni*, etc. (Q.J.G.S. 1875). On the evidence as to the Origin of existing Vertebrate Animals (lectures, *Nature*, 1876). The Rise and Progress of Palæontology (*Nature*, No. 24, 1881). The Coming-of-age of the "Origin of Species" (1880, Roy. Inst. Proc. 9, 1882).—It will be seen that Professor Huxley was a frequent contributor to the pages of the GEOLOGICAL MAGAZINE, and was one of its constant supporters since its commencement in 1864.

His last paper to the Geological Society was "Further Observations upon *Hyperodapedon Gordoni*," read May 11, 1887: see Q.J.G.S., vol. xliii, p. 675, pls. xxvi and xxvii. His latest work (published in conjunction with Dr. Pelsener) is on *Spirula* ("Challenger" Reports), 1895.

"Four kings laboured to build a mighty hall, the Hall of a Hundred Columns, at Karnak. In a century they built it, and they died; but the hall remains. Four men [Darwin, Tyndall, Huxley, Spencer], more than all others, have raised up within this century an edifice which is the crowning glory of British science; and before the century closes three of them are dead. But the edifice stands, and will stand, as a lasting monument to the power of truth and fearless investigation."—*Pall Mall Gazette.*

For further details see also "Men and Women of the Time," *The Times, Athenæum, The Standard, Daily Chronicle, Daily News*, etc., July 1st.

H. W.

ORIGINAL ARTICLES.

I.—PITTED PEBBLES IN THE BUNTER CONGLOMERATE OF CANNOCK CHASE.

By T. MELLARD READE, C.E., F.G.S., F.R.I.B.A.

(PLATE XI.)

IN a letter to this MAGAZINE of May, 1895, headed "The Indentation of the Bunter Pebbles," Mr. W. S. Gresley criticizes the summing-up of my views, given in the 'Annals of British Geology' (1892, p. 52), that "The indentation of the pebbles he considers to be the result of contact-solution, the water being retained at these spots by capillary attraction." It would have been more satisfactory if Mr. Gresley could have read my original paper[1] before penning his letter, as he would then not have assumed that I "adduce no evidence in support of the chemical theory."

Perhaps I may be permitted in the pages of this Journal to restate, and if necessary expand, my reasons for thinking that the "indentations" were not mechanically impressed, as the term rather assumes, but were simply due to solution at the points of contact. In the absence of specimens of the pebbles themselves, to thoroughly understand the question a good photograph is required, and this I have endeavoured to supply, so that my statements can be literally followed.

These are my points—

First.—If the pittings or depressions were due to mechanical pressure, the material of the pebble which was "indented" would

[1] The Trias of Cannock Chase, Proc. Liverpool Geol. Soc., Session 1891–2.

show signs of distortion. This it never does in any of the examples I have examined. A reference to the photograph will show this clearly; the depressions are perfectly sharp at the edges, and the pebble retains its external shape. The material formerly occupying the depression has been *removed*, not displaced.

Second.—The pebble that is "indented" is often harder than the pebble which indents. The "indenting" pebble is never distorted, but is frequently fractured.

Third.—Out of six indented pebbles now before me, separately collected by myself, Mr. Henry Beasley, and Mr. Edmund Dickson, when our observations were made at Cannock Chase in 1891, only one shows signs of fracture—this is *b* shown in the photograph. It is quite evident that the "fractures" are simply joints, such as may be found in many pebbles, having no indentations upon them. This pebble is a purple ferruginous sandstone or grit, almost as hard as quartzite, but not having a lustrous fracture. The interior of the "indents" is in most cases light grey, the purple colour having been discharged—another evidence of solution. There are also four quartzite pebbles from the Bunter of other localities sent me by Mr. Beasley, one of which, measuring $2\frac{1}{4}'' \times 1\frac{3}{8}''$, has been fractured and recemented by a deposit of silica which closes up about one inch in length of the crack, and is consequently subsequent to it.

Fourth.—Fractures are a sign that the material of the pebble is rigid, and that it cannot be squeezed out of shape. Their existence is to a certain extent evidence against the mechanical theory. I think no geologist will contend that the pebbles have been hardened and indurated since they became pebbles; their smooth worn surfaces show that the rock they were formed from was in the same condition as the pebbles are now.

Fifth.—The indenting pebbles *perfectly fit* the indents of the pebbles. If the indents were the result of mechanical movement this would not be likely to happen in all cases. The "indents" in the ten pebbles before me vary from $1\frac{1}{4}$ inches long by $\frac{3}{4}$ inch in width, shallow and pear-shaped, to circular pits only $\frac{1}{16}$ inch in diameter. They are of all shapes, a saucer-like shape predominating. Some of the pebbles are cemented into their places by a deposit of silica. When the indenting pebbles are removed the cup or depression is seen to be smooth, frequently having a deposit of silica over the surface, sometimes one of carbonate of iron. This deposit of silica in the case of the real quartzites is so hard that a knife will not pierce it.

Sixth.—The pittings are, in the more marked cases before me, principally confined to one side and the edges of the pebbles. The opposite side often has adherent somewhat loosely cemented sand and small pebbles. I take this, which is on the flattest side, to be the bed of the pebbles, and the pitted surface to be the top surface. Why should this be the case on the mechanical assumption? There is not much evidence of lateral pressure in the stratum in which the pebbles occur, and statical pressure one would expect to be equally effective top and bottom.

Seventh.—Let us try what the maximum statical pressure at any time on these Bunter rocks may have been. If we assume that a mile in thickness of rock at one time existed over the Bunter Conglomerate of Cannock Chase. I think it will not be above the mark. At 15 cubic feet to the ton, a column of rock a mile high would give 352 tons to the square foot. The crushing weight of granite cubes is about 720 tons per square foot on the average. Quartzite is harder.[1] If the whole pressure of the column of rock were equably distributed, it would necessarily neither crush nor disturb the quartzite. If, on the other hand, the pressure was increased by being concentrated on certain points, fracture, not distortion, would be the result, from the other parts of the pebble being unsupported. The smaller the indenting pebble the more cogent is this argument.

It is quite remarkable how numerous minute pebbles leave their marks on the very hard sandstones. Pressure applied to them in such a way as to tend to stamp them into the boulder would inevitably crush them, for it is a condition of indentation by pressure that the pressure cannot be applied in a manner to prevent crushing. There seems to be little relation between the hardness of the pebbles and the existence of the indentation. One small pebble, a specially light sandstone showing casts of fossils, has two well-formed pittings in it, and evinces no sign either of fracture or crushing.

Eighth.—But we have positive evidence that at the points of contact of the pebbles solution and deposit have been going on. In most of the depressions there is a deposit of silica which smooths the surface of the depression and unites the grains of rock. In some cases, when a joint or crack traverses the depression, this silica fills it up. The grains of silica, where they are seen in the depression of true quartzite pebbles, show like a mosaic, and appear to be flattened or cut off on their upper surface. I think it extremely probable that solution and deposit have gone on alternately. That solution of the silica has taken place, there is evidence on all hands, including the adherent sand and gravel, for solution must precede deposit. Solvent action would concentrate itself on the continually damp spots, and these are the points of contact of the pebbles, especially on the upper surface of the larger pebbles. The Bunter is a water-bearing rock, and water has a free circulation through it. Referring to the figure given by Mr. Gresley, I would respectfully submit that the branch fractures from the depression are not a proof that the depression was due to the same cause as the fracture. One pebble could not be driven into another without distortion of one or both, as already explained; and the fractures are a proof of the pebble giving way and breaking up without distortion. Also the fractures are not shown to cross the depression or indent. In none of my examples are there any radiating fractures.

[1] Quartz-rock, Holyhead, is given by Mallet as 1641·6 tons per square foot across laminations, and 900 tons parallel to laminations.—Manual of Rocks, Tables, etc., D. K. Clark, p. 631.

The following is an analysis of one of the Cannock Chase pebbles given in my original paper, and made by Mr. P. Holland, F.C.S., a member of the Liverpool Geological Society :—

Analysis of an indented pebble from Stile Cop Gravel Pit, near Rugeley.

SiO_2	79·26
Al_2O_3+TiO_2	9·60
Fe_2O_3	4·80
MnO	0·08
CaO	0·22
MgO	0·97
K_2O	2·23
Na_2O	0·19
Combined water	2·79
	100·14

As further illustrating solvent action at the points of contact, I may refer to a pebble of hard compact grey limestone (probably Carboniferous), 3 inches in diameter, $1\frac{1}{4}$ inches thick, rounded and disc-like in form, from the Bunter Conglomerate near Wolsley Bridge. There are two well-marked dish-like depressions in the stone respectively 1 inch and $\frac{3}{4}$ inch in diameter, which are coated with a thin deposit of carbonate of lime. There is also a good deal of adherent quartzose sand cemented together with carbonate of lime. Here the pebble is of *limestone*, not quartzite or sandstone; yet the same kind of action has gone on, namely, solution of the limestone at points of contact and re-deposit of the carbonate of lime. There is not the slightest sign of fracture to be seen.

EXPLANATION OF PLATE XI.

Fig. 1.—Photograph of three boulders or pebbles from the Bunter Conglomerate of Cannock Chase.

a. Very hard, fine close-grained grit, boulder, or pebble, $7\frac{3}{4}$ in. × 6 in. × 4 in., well rounded and worn. It is covered with "pittings"; the prominent circular saucer-like depression which shows in this, and more plainly in Fig. 2, is $\frac{5}{8}$ inch across. There is a shallow depression $1\frac{1}{4}$ inches long on the upper part of the pebble which is not seen. These pittings are found as small as $\frac{1}{16}$ inch in diameter, but embedded grains of quartz have been found only $\frac{1}{50}$ inch in diameter. The pebbles, where entirely clear of adherent sand, are quite sharp at the edges, as may be seen on examining the plate with a lens. The surface of the pittings is mostly granular, but in some cases has a deposit of silica upon it. The projections above the general surface of the boulders are either whole pebbles, broken or decayed pebbles, or groups of pebbles with sand between cemented thereto. The cemented sand sometimes makes a rim round the depression. Most of these small pebbles, with which the large one is roughened, are counter-sunk or embedded in the boulder, and if removed would disclose pittings. They are generally very firmly cemented into the depressions with silica, and fit them exactly. The opposite side of the boulder has a good deal of adherent sand and gravel, but not many "pittings," which are principally confined to the surface shown in the photograph and to the edges. (Stile Cop.)

b. Hard, fine-grained, purple-brown coloured grit boulder, or pebble, full of joint planes, along three of which it has split; these broken joint surfaces are covered with ferric oxide. The "pittings" are very numerous, and are of a lighter colour than the surface of the pebble, which is well water-worn. One of the pittings is canoe-shaped, $1\frac{1}{4}$ inches long by $\frac{1}{4}$ inch wide. There are small pebbles adherent and imbedded; in this and most other respects the description of boulder *a* applies to boulder *b*. (Stile Cop.)

c. Purple-coloured quartzite boulder or pebble, 5¾ in. × 4¼ in. × 3 in., a flattened oval in form, covered with depressions of a less decided type than *a* or *b*. The surface of the depressions are white from a deposit of silica therein. There is only one small adherent bit of gravel. The opposite side to that shown in the photograph has no "pittings," but a good many small adherent patches of sand about ⅛ inch in diameter, apparently indicating the points of contact of small pebbles or gravel, now removed. There are depressions on the edges. (Stile Cop.)

FIG. 2.—The boulders shown are the same as in Fig. 1, but arranged in a way to get the light from another direction to show up some of the features not properly developed in Fig. 1.

II.—Two Occurrences of Radiolarians in English Cretaceous Rocks.

By George E. Grimes, B.Sc., A.R.S.M., A.R.C.S., F.G.S.

THE opinion that fossil Radiolarians were entirely limited to Tertiary strata prevailed until a comparatively late period. Prof. Ernst Haeckel, in his monograph on the Radiolaria, published in 1862, says "that the Radiolarians made their appearance for the first time during the Tertiary period"; and Ehrenberg, as late as 1875,[1] defined all the Polycystina-bearing rocks as Tertiary.

For a long time the Cretaceous rocks yielded but few remains of Radiolarians, and even as late as 1885 Dr. Rüst remarked on the poverty of Radiolarian remains in Cretaceous strata.[2] Since that date, however, chiefly owing to the patient and laborious researches of Dr. Rüst, a large number of species have been obtained from various rocks on the Continent, principally from the Lower Cretaceous.[3]

Prof. Sollas was the first to notice Radiolaria in the Cretaceous rocks of this country, and in his paper "On Greensand Sponges and Foraminifera"[4] he says: "Polycystina of various genera also occur; forms resembling *Haliomma* may be noticed of somewhat frequent occurrence." Prof. Sollas promised a full description of these forms later, but I believe it has not yet appeared.[5]

In 1888 Dr. Rüst[6] described two species—*Dictyomitra anglica* (Rüst) and *Dictyospyris chlamyden* (Rüst)—from flints of Senonian or Turonian age from England; but the locality is not given.

The object of this paper is to describe two fresh occurrences of Radiolaria in our Cretaceous rocks.

One is in the Fuller's Earth Rock Bed in the Lower Greensand, between Redhill and Nutfield, Surrey. This bed lies directly on

[1] Fortsetzung der mikrogeologischen Studien: Abhandl. der k. Akad. der Wissen. Berlin, 1875.

[2] Beiträge zur Kenntniss der fossilen Radiolarien aus Gesteinen des Sura: Palaeontographica, vol. xxxi.

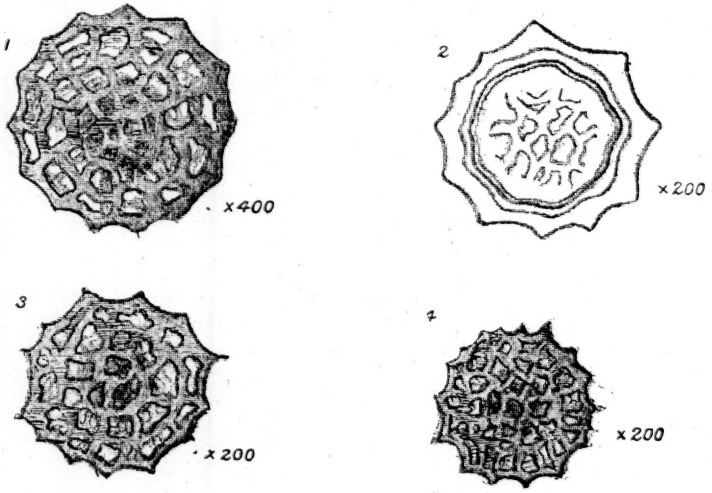
[3] Beiträge zur Kenntniss der fossilen Radiolarien aus Gesteinen der Kreide: Palaeontographica, vol. xxxv.

[4] Geol. Mag., Vol. X, 1873.

[5] It should also be mentioned that Dr. Wallich recorded the occurrence of several genera of Radiolaria in the interior of Chalk flints in 1883 (Ann. and Mag. Nat. Hist., July 1883, p. 52).—Edit. Geol. Mag.

[6] Beiträge zur Kenntniss der fossilen Radiolarien aus Gesteinen der Kreide: Palaeontographica, vol. xxxv.

top of the Fuller's Earth, and is mapped by the Geological Survey as equivalent to the Sandgate Beds of East Kent; but this is disputed by some geologists. As the rock forms the surface of the dip slope of the Lower Greensand at this place it has been considerably denuded, so that its thickness varies from 0 to about 27 feet. When examined in thin sections with the microscope the rock is found to be composed chiefly of broken sponge spicules, and with these Foraminifera (mostly infilled with glauconite and in many cases having the original test), glauconite grains and casts, fragments of tests of Echinodermata, some grains of quartz, and more or less iron oxide. In a few slides globate spicules of *Geodites* were also noticed. The silica in the sponge spicules mostly assumes in a greater or less degree the globular form, which was first noticed by Dr. G. J. Hinde in his paper "On Beds of Sponge Remains in the Lower and Upper Greensand."[1]



Radiolarians from the Greensand of Redhill and Reigate.

Fig. 1.—*Carposphæra Neocomiensis*, sp.n. From the Fuller's Earth Rock Bed, Lower Greensand, Redhill, Surrey. × 400.

Fig. 2.—*Haliomma* sp.? From the Fuller's Earth Rock Bed, Lower Greensand, Redhill. × 200.

Fig. 3.—*Carposphæra*, sp. From the Upper Hearthstone, Upper Greensand, Reigate Hill, Surrey. × 200.

Fig. 4.—*Carposphæra*, sp. From the Lower Hearthstone, Upper Greensand, Reigate Hill. × 200.

Radiolaria were most numerous in the rock from the pit at the top of Redstone Hill and on the south side of the road between Redhill and Nutfield, where there were as many as seven or eight in a section of a little over a square inch in area. The Radiolarians in this bed mostly belong to the genus *Carposphæra*, Haeckel, with

[1] Phil. Trans. Royal Soc., part ii, 1885, p. 427.

two concentric spherical lattice shells (outer cortical and inner medullary). As the lattice shells are now more or less infilled with foreign material the lattice structure of the inner one is not always visible. One of the best defined of these forms is represented in Fig. 1, and as it appears to be new I propose to name it *Carposphæra Neocomiensis*. The diameter of the cortical shell is ·07 mm. (Fig. 1), and the breadth of the medullary shell ·032 mm. There appear to have been originally eight or nine rays connecting the two shells. The lattice mesh of both shells is irregularly polygonal.

Haliomma species? Fig. 2.—As seen in section the boundary is irregularly circular, with numerous cusps, probably the bases of spines. The inner border, as seen in figure, was probably the boundary of the inner test. In the central area there are some faint indications of lattice structure. This organism was found on the thin edge of the rock section. Diameter ·139 mm.

The second occurrence of Radiolaria is in the Upper Greensand at Colley Farm, Reigate. The section at present visible in the pits is, in descending order—

Chalk Marl.	ft.	in.
Greenish Sand (equivalent to chloritic marl)	7	6
Chert	0	6
Soft Hearthstone	6	0
Chert	0	6
Hard Hearthstone	5 to 6 feet.	
Fire and Building Stone	6	0

The Radiolaria were found both in the Upper and Lower beds of Hearthstone. These beds in thin sections under the microscope exhibit a similar composition to that of the Fuller's Earth rock previously described, but the constituent fragments are smaller in size. The beds are made up of broken sponge spicules, grains of quartz, glauconite grains and casts, Foraminifera mostly infilled with glauconite and flakes of mica. The globular form of silica is, however, more developed than in the Fuller's Earth rock, and when sections are examined with the $\frac{1}{4}$ inch objective the globules are seen to make up a large proportion of the slide. The Radiolaria in these beds are not so well preserved as those in the Fuller's Earth rock, but they appear to belong to the same genus—*Carposphæra*.

Fig. 3 represents one of these from the Upper Hearthstone; the diameter of the cortical shell is ·145 mm. Fig. 4 represents a form from the Lower Hearthstone; the diameter of the cortical shell is ·1 mm.

I have to thank Mr. F. Chapman, F.R.M.S., for advice and assistance in the preparation of this paper, and for the excellent figures which accompany it. I wish also to thank Dr. G. J. Hinde, F.G.S., for examining the specimens and advising me on some difficult points; also Professor Judd, F.R S., for kind advice and facilities for making some of the preparations.

III.—Notes on "The Great Ice Age" in Relation to the Question of Submergence.

By Dugald Bell, F.G.S.

(*Continued from p.* 326.)

III. The Second Glaciation.

"EVEN if marine life had been prolific, and the old sea-bottom more or less well covered with sedimentary deposits, it does not follow," says Dr. Geikie, "that the Boulder-clay of the succeeding *mer de glace* should now contain any shells" (p. 141).

This is what we have now to consider—that the marine deposits of the submergence, however abundant, may have been swept away by the succeeding glaciation so completely as to leave no trace in the Upper, or post-submergence, Boulder-clay.

Now, we may assume that, in the gradual subsidence and re-emergence of the land ("leaps and bounds" being discarded), the sea would for some time occupy all possible levels up to the supposed maximum limit, and would, on the premises now granted, leave traces of its presence, and of its "prolific" organic life, more or less abundantly at them all. Indeed, such traces would, so to speak, have a double chance of being left—first, during the submergence, and again, during the re-emergence. Further, at every successive level the sea would go into innumerable bights and bays, ravines and sheltered hollows of the land where, from their very nature, subsequent ice-sheets could not follow it.[1] This is obvious enough in itself, but we are glad to have it also on Dr. Geikie's authority. Speaking of the beds of silt, sand, and gravel which in many places are intercalated in the "Till"—indicating a time when the great ice-fields had receded "so far at least as to uncover the lowland tracts and valleys"—he shows that "during the Glacial period the ice-sheet, which followed the lines of the principal valleys, must frequently have crossed the lateral and tributary valleys nearly at right angles. In the main valleys," he continues, "the glacier would exert its full influence, but it would not be able to do so in the narrow lateral valleys and ravines; the ice and Till would merely topple into the glens referred to, and gradually choke them up, and the main mass of the glacier would then pass over the whole .... In such narrow glens, then, any silt, sand, or gravel that had gathered during the absence of the ice-sheet would not be so likely to be ploughed out" (pp. 108–9).

Accordingly, in many hundreds of such little tributary glens all over the country, and at many different levels in them all, marine deposits, if the sea had ever been there, would have a good chance of being preserved.

And that chance, we have to add, would be considerably increased by the very circumstance of these deposits being mainly spread out over the low grounds, where the erosive power of ice-sheets is least, and where, instead of "sweeping out," they tend rather to

[1] See Trans. Geol. Soc. Glasgow, vol. ix, pp. 109, 110.

accumulate débris. Thus Dr. Geikie goes on to say—"I do not believe it was necessary for the preservation of intercalated deposits that they should always have occupied a hollow or depression sheltered from the full sweep of the ice-flow. The great thickness attained by the Till in broad open lowland districts" [this is different from the "narrow belt of coast-land" formerly spoken of] "shows that over such areas there was a tendency for the Till to accumulate, probably owing to a diminished rate of ice-flow. . . . Wherever the flow of the ice-sheet slackened, there would necessarily be less erosive action, and therefore a good chance of pre-Glacial and inter-Glacial beds being preserved" (p. 112; see also pp. 74–5).

We leave Dr. Geikie to reconcile these clear and distinct statements with his arguments for the complete erosion and removal of the marine sediments by the "second glaciation." How could the ice-sheets of that period, while thus largely sparing the "Lower Till" and the overlying fresh-water deposits, pick out and carry away so completely, and from all the low-lying and most sheltered positions in the country, every trace of the marine deposits, "prolific" in organic remains, which must have been there had the sea reached the level, or anything like it, which he contends for? Does he not require far more of the ice than we do? *We* suggested that it may have transported some portions of marine clay and sand a few miles inland from the sea; *he* supposes that it may have removed hundreds of square miles of the same materials into the sea, sometimes over great distances and across every inequality of ground, and left scarcely "a wrack behind"! Truly we may say, "glacier-ice has played many strange freaks, but one may be excused for doubting whether it is equal to this remarkable performance" (p. 141).

Indeed, we do not require to go beyond this individual section at Clava to show how untenable Dr. Geikie's position is. Here, in this upland valley of the Nairn—where, be it noted, there is every evidence that the action of the ice was *along* the valley, and not *across* it,[1]—here are some 36 feet of partly stratified deposits overlaid by a great thickness of Boulder-clay. The "second glaciation" must evidently have been in great force here, to have laid down more than 40 feet of this "Upper Till." Yet it did not wholly "clear out" or "sweep away" the previously existing beds of "fine sand" and "blue clay"; it merely covered them up—at least 36 feet of them—and passed on! What ground is there for saying that it would do otherwise in hundreds of similar, or far more sheltered, localities all over the country?

But if we wish another instance to confirm the view which we are now maintaining, it is furnished to our hand in that immediately afterwards referred to by Dr. Geikie (p. 142), viz.—

The Strathendrick Shelly Till, which was described by Mr. Jack, of the Geological Survey, a good many years ago, as occurring

[1] "The River Nairn runs in a north-easterly direction, and the ice-markings on the floor of the valley indicate that the later ice-flow adhered pretty closely to the direction in which the river now flows" (Trans. Inverness Sci. Soc., vol. ii).

in the lower part of the Endrick valley, a few miles beyond the south-east end of Lochlomond, and at various elevations from 80 to 320 feet above the sea.[1] This shelly Till or Boulder-clay Mr. Jack attributed to the action of a glacier which occupied the bed of Lochlomond after a moderate submergence had converted it, for a time, into an arm of the sea. We say "a moderate submergence," for a little over 20 feet would suffice, as Mr. Jack himself pointed out. But as a laminated clay containing some marine shells and a fragment of a deer's horn was discovered in the same neighbourhood at a height of 108 feet, and assumed to be "in place" (which we think is open to question), Mr. Jack inferred a "minimum submergence" of that amount, 108 feet. This, however, he took care to say, was not to be confounded with the greater submergence in which he then believed, and which he assigned to a later date. After the minor or "minimum" submergence referred to, Mr. Jack's view was that "a large glacier filled up the lake, covered the islands, and climbed the rising ground beyond to the height of at least 320 feet," carrying up with it, from the bottom of the lake, this shelly Till.

Now, this instance evidently tells forcibly against the theory of the total demolition and disappearance of marine remains as a certain result of the "second glaciation." Accordingly, Dr. Geikie suggests that this Strathendrick Till "belongs to a later date than any of the shelly Boulder-clays" he had been referring to; and that it "appears to have been deposited by a local glacier." As to *time*, this is quite opposed to Mr. Jack's view, which was that this shelly Till preceded the "great submergence." It differs from Mr. Jack's view, also, as regards the *means* or *agency*. What is meant by a "local glacier"? We naturally think of one of the smaller ice-sheets which continued to linger in the upland valleys and glens, around the borders of the mountain districts and their gradually diminishing snow-fields, after the extreme period of glaciation had passed away. But Lochlomond is not in any such upland glen or valley; the bottom of it is in great part under sea-level, in parts as much as 500 to 600 feet. The valley is, moreover, 24 miles, or, with its continuation to the head of Glen Falloch, some 36 miles, in length; and there is no point nearer than the head of that glen, or still farther away up Strath Fillan, among the group of high mountains there, which can be indicated with any probability as the place of origin of its glacier. All the striations that have been observed in the district lead to that quarter. If, then, a glacier 40 or 50 miles in length, and a good deal over 1000 feet in thickness,—for such it must have been to have filled up the lake and carried its shelly Till to an elevation of 320 feet above it,—if such a glacier be called "local," it can only be in the sense of not being part of a "general" ice-sheet, though apparently very near it. For, when such a glacier existed in Lochlomond, we may safely say that every arm of the sea all around our coasts would have its glacier of corresponding dimensions, each separated

[1] Trans. Geol. Soc. Glasgow, vol. v.

from its neighbours (where it was separated) by only very narrow ridges of land.

Mr. Jack, at least, believed that "the shelly Till was produced by a glacier which had its gathering ground partly in the region drained by the Fillan"; because "a glacier nourished solely in the Lochlomond valley could not have climbed the rising ground between the Leven and the Endrick to the height (320 feet) to which the shells have been traced." This seems undeniable.

We ask, then—If this second glaciation did not utterly destroy and carry away out of sight the marine shells taken up from the comparatively limited area of Lochlomond, but left abundant traces of them within a few miles of the loch, what reason is there for holding that it—or any similar glaciation—would make a "clean sweep" of all relics of marine life from every bay and inlet, ravine and valley all over the country, that would be occupied by the sea during the supposed submergence of over 500 feet?

We accordingly claim this Strathendrick shelly Till as a strong corroboration, and indeed a practical proof, of our contention that had there been such a submergence, the "Upper Boulder-clay," held to be due to a succeeding glaciation, would undoubtedly, as a rule, be charged more or less abundantly with marine organic remains—Q. E. D.

IV. The Clava "Shell-Bed."

From what has been said, it appears that the absence of all traces of the sea at a similar level, and at many approximate levels, all round the country, and the absence of relics of marine life, more or less abundant, in the Upper (or supposed post-submergence) Boulder-clay, are insuperable difficulties against accepting the alleged submergence of 500 to 600 feet—that is to say, difficulties which have resisted all attempts to explain them away.

The difficulties against the ice-transport theory, on the other hand, must be shown to be greater than those against the submergence theory, before we can be asked to accept the latter as in any degree a satisfactory account of the phenomena.

Now let us take this special instance of *Clava*. There are several points connected with it which seem to require careful consideration. It may conduce to clearness if we here repeat the section—

Above Sea.		Feet.
566 feet.	Surface soil and upper light-brown Boulder-clay; many of the stones striated	43
523 feet.	Fine yellowish-brown sand, very compact; a few very small stones	20
503 feet.	Dark-blue or grey shelly clay, slightly bedded, very little sand or gravel; almost free from stones, except in lower part . .	16
487 feet.	Coarse gravel and sand, and brown stony clay. (Lower Till, partially reassorted?)	36½
450 feet.	Rock, gritty Old Red Sandstone	

We quote the following summary from the carefully written "Note" of the minority:—

"The ice-transport theory, therefore (whatever difficulty may attach to it), has at least this point in its favour, that the deposit is quite in the track of ice which would almost certainly pass over part of a former sea-bed in its progress. It has

also this other point, that the shelly clay consists almost wholly of materials derived from some distance, differing from those in the immediate neighbourhood, and from the Boulder-clay and gravel above and below it. Further, though the clay itself suggests deposition in deep and comparatively still water, the shells and other organisms it contains are almost entirely of littoral species; and though the stones in it are in general rounded and water-worn, some distinctly striated are associated with them, and all occur promiscuously imbedded in this fine unstratified clay, without, as a rule, even a streak of accompanying sand or gravel. Mere submergence seems inadequate to account for these facts. And we venture to say that to assume, first, a submergence of over 500 feet, then a re-elevation to about the old level, with a return of glacial conditions much the same as before, is to hang an immense series of changes upon the (as regards interpretation) more or less doubtful evidence before us." (Report, p. 31.)

Let us now look at some of the difficulties advanced on the other side. One is the well-preserved condition of many of the shells. Numbers of them, it is admitted, are crushed and broken; but many of them, it is pointed out, are whole, some having the epidermis entire, and none, so far as observed, showing any trace of ice-markings or abrasion. All this, however, is paralleled by numerous known instances in which even delicate shells have been transported uninjured by ice, so that it cannot be said to be at all conclusive.

Other points that are urged against the ice-transport theory are the "extent" of the bed, its "horizontality," and its exhibiting "no trace of deformation or disturbance" ("Great Ice Age," p. 141). But in regard to these points opinions will differ as to the clearness or decisiveness with which they are proved by the Report. Take, first, the "extent." We admit, of course, the fact that a "blue sandy clay" was found at 30 yards east and 160 yards west of the main pit, and also in bores at other two points between. Nor are we disposed to make much of the accompanying fact that only one of the bores showed any traces of shells. We are willing to grant that it was probably the same clay, and probably continuous. We remark, however, as very likely having some bearing on its mode of formation, that it seemed to thin out very unevenly on both sides of the main pit, and at a certain distance on either side it ceased altogether. At the main pit it was 16 feet in thickness; at 30 yards to the east it was but $2\frac{1}{2}$ feet; and at 61 yards in the same direction it was not found. Again, at 160 yards west, it was only 15 inches in thickness; at 197 yards it was not found. It thus appears to be a very irregular mass, and restricted within certain definite limits. Certainly it is not "horizontal," the difference in level between its two extremes being about 20 feet. And as certainly it has been subjected to great compression and crushing—whether during its deposition, or since, is a question.

Now (1) we might ask whether the known instances in which masses of clay and fragile slabs of Chalk, etc., of considerable dimensions (some upwards of 180 yards in length) have been transported uninjured by the ice, are not sufficient in some degree to neutralise, or lighten, any objection resting on the apparent or "proved" extent of the "bed"?[1]

[1] See Mellard Reade on "Masses of Chalk imbedded in the Drift of Cromer" (Q.J.G.S., vol. xxxviii, 1882). Also "Great Ice Age," p. 338.

(2) But if any think otherwise, we revert to what we have already urged as to the *limits* of the deposit (which have been proved with at least equal clearness) being a far greater objection to the theory of a submergence.

(3) We would also repeat that the objection drawn from the extent of the "bed" proceeds upon an assumption to which we are by no means restricted, that the deposit was all transported by the ice at once or *en masse*.

At this stage we wish briefly to emphasize one of the points alluded to in the minority's Note, viz.: the striking difference between the composition of the shelly clay, and that of the "rough gravel" below and the Boulder-clay above it.

Be it remembered that the whole section rests on that belt of Old Red Sandstone which sweeps along the southern shore of the Moray Firth, from Loch Ness to Fochabers and Buckie. It is surely a remarkable fact that while the stony materials of the deposits between which the shelly clay occurs, consist of from 70 to 80 per cent. of Old Red Sandstone, and only 20 to 30 per cent. of the more ancient rocks (schists, gneisses, etc.) of the neighbourhood; in the clay itself the proportions are nearly exactly reversed—70 to 80 per cent. of the more ancient rocks, and only about 20 per cent. of the Old Red Sandstone! The materials forming the clay have thus been mainly derived from a greater distance than those forming the rough gravel, or "Till," on either side of it. The difference extends even to the sand taken from these separate parts of the deposit. The sand above the shelly clay, and that washed from the coarse gravel below it, is light in colour, consisting mainly of "well-rounded particles of quartz derived apparently from the adjacent Old Red Sandstone." But the sand contained in the shelly clay itself is "dark or dark-grey in colour, and contains much black mica apparently derived from the disintegration of gneissose rocks" (Report, p. 21).

On the theory of a submergence, it seems impossible to explain the sharp intercalation of a deposit of this kind, so thoroughly different from its immediate surroundings.

It has been suggested, indeed, by an esteemed member of Committee, that as the submergence increased the Red Sandstone of the district was all brought under water, and that then the "blue shelly clay," resulting from the waste of the schistose and gneissose rocks that remained exposed, was deposited. This, however, as far as we can see, will not hold good. The bottom of the shelly clay is 487 feet, and its top 503 feet above the sea; whereas the sandstone in the immediate neighbourhood reaches a height of 700 to 1000 feet, and further to the south-west it ascends to 1500 feet. And from that direction the principal flow of sediment into the supposed ocean basin would undoubtedly take place. Therefore, long before the sandstone was completely submerged,—of which, by the way, there is not the slightest independent evidence,—there would be such a depth at Clava as is inconsistent with the prevailing littoral character of the shells. Indeed, the limited extent

of shallow shore there could at any time be at Clava, is a difficulty in this respect. Further, on this theory the transition ought to be gradual; as the area of the sandstone narrowed a smaller and smaller proportion of sandstones should appear, till at length the schists predominated. But here the change is sudden and complete at the very bottom of the clay. As the Report says, "The boundary line between the shelly clay and underlying gravel is clearly defined. There is no intermingling of the two deposits" (p. 4). Similarly, the line of junction between the shelly clay and the overlying sand is "clearly defined" (p. 3). There seems thus to have been a sudden and complete change of conditions during the deposition of the shelly clay, not to be accounted for by submergence—not even by a "rapid and brief" submergence, overlooking for a moment the intrinsic improbabilities of such a supposition, and its contrariety to the other facts of the case. For, as Mr. David Robertson remarks in his valuable Report, "the clay seems to indicate deposition in still water, showing no traces of strong currents." There was very little sand in it; 90 per cent. being fine mud or silt. This is not consistent with the theory of a "rapid and brief" submergence. But how, in any case, could there be "still water" and no strong currents here if this were then a shore of the northern sea, and immediately adjacent to, nay, actually swept by, the strong tides and currents that must then have flowed to and fro through the Great Glen? We can scarcely imagine a locality where, during the supposed submergence, there would be less likelihood of "still water" than at Clava.

Thus, the more it is considered the more do difficulties accumulate round the theory of a submergence to this extent, which, besides, there is absolutely no independent evidence to support. And all these difficulties and objections, if real and well founded, are so many arguments in favour of the ice-transport theory, which (as Professor Ramsay admitted with regard to the analogous cases of Holderness, etc.) is the only alternative.[1]

We think we may now claim to have shown that the ice-transport theory, as applied to this Clava deposit, is, on the whole, much more probable than the theory of submergence. This is all that is required to vindicate the position of the minority. They did not undertake to say what was the precise mode in which the deposit was conveyed or accumulated; they merely expressed their opinion, "with all deference," that the submergence theory was not satisfactory, and that the Committee had "not yet reached a solution of the difficulties" connected with the deposit.

Here, therefore, our remarks might end. But the present writer is now prepared, on his own account, to offer a further suggestion which may perhaps lighten the difficulties still felt, in some respects, to exist. We have said that, if transported by the ice, it does not "necessarily follow" that the whole of this deposit was conveyed simultaneously, or *en masse*. Briefly, our idea is that it may have

[1] Phys. Geol. and Geog. of Great Britain, p. 157 (3rd edition).

been conveyed very gradually, and deposited in an *extra-glacial* lake, formed at this point along the side of the ice-sheet, into which part of the materials being carried by the latter (fine mud, rounded stones, shells, etc.) dropped and were accumulated. The great ice-sheet which issued from Loch Ness, and turned eastward along the base of the Monadhliath hills (as it was compelled to do by the blocked condition of the Moray Firth and the North Sea beyond) would not always, or at all points, cling closely to the base of these hills. There are bends and curves, and small lateral valleys, which it would pass straight across, blocking the drainage, under certain conditions, and so forming lakes of the nature indicated.

The physical configuration of the valley at this point bears out the suggestion. Clava is situated right in front of such a bay or recess in the hills as we have spoken of, which may very probably have been barred by the ice-sheet and converted for a long time into such an "area of deposition" as described. This will be seen quite clearly by examining the contour lines of the locality, in any Ordnance Map.

Several of the puzzling features of the deposit may thus be more or less clearly explained. The complete change which would take place when the lake was formed, and while it was being gradually filled up; the accumulations of this thick mass of fine unstratified silt or mud; the rounded and striated stones dropped into the heart of it with scarcely any trace of sand or gravel; the shells, also, of mixed kinds, but chiefly of shore species, occurring in it with a like absence of the ordinary shore débris; the basin-like form of the clay itself, so far as ascertained; the overlying bed of fine sand, indicating a gentle, steady current, or movement of the water, when an outflow was established;[1] and finally, the Upper Boulder-clay, showing a subsequent extension, or change of direction, of the ice-sheet covering up the whole;—the various parts of the section seem to fall into order, and become in some degree intelligible. Various confirmatory particulars might be adduced, on which, however, we cannot at present enter. Meantime we may say with the old Latin poet—

> Si quid novisti rectius istis,
> Candidus imperti; si non, his utere mecum.

We have only to add that, while thus maintaining our ground against such powerful odds, we trust we have written nothing inconsistent with the respect which we have always felt towards Dr. Geikie for his long and eminent services in this department of Geology.

[1] "A velocity of 6 in. per second will lift fine sand, 8 in. will move sand as coarse as linseed, 12 in. will sweep along fine gravel," etc.—Sir J. Lubbock.

IV.—On the Effect of the Glacial Period in Changing the Underground Temperature Gradient.

By Charles Davison, M.A., F.G.S.;
Mathematical Master at King Edward's High School, Birmingham.

In his recent work on "Controverted Questions of Geology" (p. 159), Professor Prestwich remarks that the intense cold of the Glacial period may still be perceptible in the underground temperature gradient; that "to a certain depth the rate of cooling is now abnormally slow, owing to the excessive refrigeration the crust then underwent." The suggestion is a valuable one, and I propose to test it in the present paper by estimating roughly the change which the temperature gradient may have experienced since the close of the Glacial period.

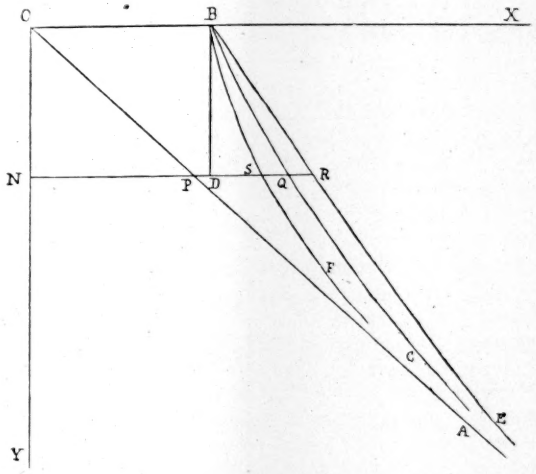
In order to avoid confusion, it may be well to state precisely the meaning which is here attached to the term "temperature gradient." If the temperature increase 1° F. for every 50 feet of vertical descent, the gradient is taken to be 1° per 50 feet or $\frac{1}{50}$° per foot, but not 50 feet per degree. If the gradient in course of time were to change to 1° in 60 feet, it will be said to have decreased, whereas in the latter case it would be said to have increased.

The first point to be determined is the rise of mean annual temperature which has taken place since the Glacial period. The present value of the mean annual temperature in England varies between 45°·6 (Dartmoor), and 51°·9 (Helston), the average over the whole country being 49°·5 F.[1] With regard to the Glacial period, we are concerned, not so much with the mean annual temperature of the air at that time, as, in glaciated districts, with that at the base of the ice-sheet; and this, it is probable, underwent but little change during the whole time the ice-sheet lasted, however intense the cold at its surface may have been. For, if the thickness of the ice exceeded a certain limit, the temperature at the bottom, owing to the rise of the isogeotherms, would be maintained at the melting-point corresponding to the pressure of the overlying ice. With a mean temperature of 0° F. at the upper surface of the ice, and a gradient in the rock below of one degree in 60 feet, Mr. Fisher finds this limiting thickness to be 714 feet.[2] With a gradient of one degree in 50 feet, the limit would be 596 feet. This may, however, be an under estimate, for it does not take the depth of the "surface of invariable temperature" into account. Moreover, while the temperature at the depth of this surface is in rock the mean annual temperature of the locality, it may be less in the case of an ice-sheet whose temperature can never rise above the melting-point, and is therefore incapable of following the oscillations of the external temperature above that point. The effect of the omissions is not, however, very serious, involving indeed in some cases an increase in the above estimate, though not, it may be, a great one.

[1] This is the average of the annual means at 57 stations given by Dr. A. Buchan in his great work on "Atmospheric Circulation," "Challenger" Reports, Physics Chemistry, vol. ii, pp. 196–199.

[2] Phil. Mag., vol. vii, 1879, p. 385.

Considering the low mean temperature assumed to exist at the surface, there is probably no occasion to alter the above estimate of the limiting thickness. But if we double it, and take it at 1200 feet, we shall be well on the safe side; and there is no doubt that for a very long period the ice-sheet in many places attained a greater thickness than this. Now the melting-point of ice is lowered 1° F. by the pressure of every 2730 feet in the vertical thickness of overlying ice, so that the temperature at the base of the ice-sheet may have been as low as 30° or even 29°, but can never under any circumstances have risen higher than 32°. We cannot err very greatly, therefore, in supposing the surface of the ice-covered ground during the whole Glacial period to have remained at a uniform temperature of 30°·5. The subsequent rise of temperature may thus be estimated at 19° F.



All geologists are agreed in attributing a duration of several, or many, thousand years to the Glacial period. If this be correct, the temperature at its close must for some miles have increased almost uniformly with the depth. Thus, if in the accompanying figure O represent a point on the earth's surface, O X a horizontal, and O Y a vertical line, the temperature-curve O P A near the surface must have been almost a straight line, N P representing the temperature at the depth represented by O N, or, rather, the excess of temperature above 30°·5. If O N were taken to represent one foot, then N P would represent the temperature gradient.

Now let us suppose that, at the end of the Glacial period, the mean annual temperature at the surface rose *suddenly* by 19°; and let the line O B represent this amount. Then, at some subsequent time, the temperature-curve would be of the form B Q C, where N Q represents the temperature at the depth O N, the lines O P A, B Q C joining at the point where the change of temperature ceases at

the particular time to be sensible. The excess of temperature at the depth O N above that at the surface is represented by D Q, which is obviously less than N P, so that the temperature gradient is diminished at all points to which the heating has become perceptible.

With a further lapse of time the heating effects will have penetrated to a greater depth, and the temperature-curve will have assumed the form B R E. Here, the excess of temperature at the depth O N above that of the surface has increased (D R being greater than D Q); consequently the temperature gradient has also increased, and, continuing to do so, will in time attain a value not differing very greatly from that which it had just before either the close or the commencement of the Glacial period.

Before proceeding further it should be remarked that the inaccuracy of the assumption of a *sudden* change of surface temperature must of course affect all the numerical results hereafter obtained. But, as we do not know the law according to which the rise of temperature took place after the disappearance of the ice-sheet, it seems to me allowable to make the assumption, especially as the provision of *exact* numerical results is not the purpose of this paper. Moreover, if the change were gradual but continually in one direction, the temperature-curve would be of the form B S F, and this would result in a still further decrease of the gradient, so that the change of gradient would in reality exceed that found on the assumption of a sudden rise of surface temperature.

The last element required, and that about which the greatest uncertainty exists, is the time that has elapsed since the close of the Glacial period. On account of the small amount of denudation accomplished, the tendency of opinion has recently been to reduce the estimates of the length of this interval. Prof. Prestwich even puts it at only eight or ten thousand years. The maximum figure is probably that given by Croll of about eighty thousand years.

Under the circumstances, it seems best to calculate the change of temperature gradient for a series of different intervals. In order to give definiteness to the results, I have taken the present gradient at one degree in 50 feet, and, in Table I, have expressed the value the gradient must have had at the close of the Glacial period on the assumptions stated above, for different estimates of post-Glacial time from 5000 to 40,000 years, and for different changes of the mean annual temperature from 16° to 22°.[1]

Thus, if the rise of mean temperature since the Glacial period be 19°, and if the interval since its close be only 9,400 years, the

[1] The calculations are performed as follows:—If b be the rise of mean annual temperature at the surface in degrees, κ the conductivity of rock expressed in terms of its own capacity for heat, t the number of years since the change of temperature took place, then the change of gradient is $b \div \sqrt{\pi\kappa t}$ (Rev. O. Fisher, Phil. Mag., vol. xxxiv, 1892, p. 339). This value, it should be remarked, is independent of the previous gradient. Taking Lord Kelvin's value for κ, namely 400, the above formula becomes $\cdot 02821\, b \div \sqrt{t}$. If $b = 20$ and $t = 10000$, $b \div \sqrt{t} = \frac{1}{5}$, and the change of gradient is $\cdot 00564$ degree per foot. But the gradient now is taken to be $\cdot 02$ degree per foot, so that, at the end of the Glacial period, it must have been $\cdot 02564$ degree per foot, or one degree in 39 feet.

gradient just before that close must have been one degree in 39 feet; if the interval be 15,000 years, one degree in 41 feet; and if 27,000 years, one degree in 43 feet. Or, again, at the bottom of a mine half-a-mile below the "surface of invariable temperature," *i.e.* about 2700 feet deep, the temperature would be 53° above that at the surface with a gradient of one degree in 50 feet, 68° above with a gradient of one degree in 39 feet, 65° with one of one degree in 41 feet, and 61° with one of one degree in 43 feet. In any case, it follows that the gradient now differs very sensibly from the value it had at the close of the Glacial period.

TABLE I.

Showing Number of Feet of Vertical Descent corresponding to a rise of 1° F. in underground temperature at the close of the Glacial period.

Number of Years since end of Glacial period.	Assumed Increase of Mean Annual Temperature at the Surface.						
	16°	17°	18°	19°	20°	21°	22°
5000	37·9	37·3	36·8	36·3	35·7	35·2	34·8
10000	40·8	40·3	39·9	39·4	39·0	38·6	38·2
15000	42·2	41·8	41·4	41·0	40·6	40·2	39·9
20000	43·1	42·7	42·4	42·0	41·7	41·3	41·0
25000	43·8	43·4	43·1	42·8	42·4	42·1	41·8
30000	44·2	43·9	43·6	43·3	43·0	42·7	42·4
35000	44·6	44·3	44·0	43·7	43·5	43·2	42·9
40000	44·9	44·7	44·4	44·1	43·8	43·6	43·3

The question whether the present gradient retains any traces of the Ice Age depends upon several conditions, such as the mean annual temperature existing before the Glacial period, and the relative lengths of the Glacial period and the subsequent interval. Obviously, this is a question to which it is difficult, perhaps impossible at present, to furnish any decisive answer. If the pre-Glacial mean temperature were the same as it is now, if the glacial conditions commenced and ended abruptly, and if, moreover, the Glacial and post-Glacial periods were of equal duration, then the gradient now would be the same as it was before the Glacial period, that is assuming the normal gradient[1] to have undergone no sensible change during the interval. Supposing the first two of the above conditions satisfied, however, it may be worth while giving a few numerical estimates of the change of gradient depending on different assumptions as to the lengths of Glacial and post-Glacial times. These will be found in Table II, which supposes that the present gradient is one degree in 50 feet, and that the mean annual temperatures were constant during both periods and differed by 19°.

As the general opinion seems to be that the duration of the Glacial period was much greater that that of the succeeding interval, it is therefore possible that some relics of the Ice Age are still to be found in the present temperature gradient.

[1] The gradient due to the temperature of solidification, the surface temperature having remained constant ever since.

A few practical applications of these results may be mentioned in conclusion.

(1) The usual method of finding the average temperature gradient over large areas may be incorrect, unless it is known that all the stations have not recently experienced the effects of a considerable change of mean annual temperature. For instance, the change of gradient in Scotland and the north of England since the Glacial period may be different from that which has taken place in France or Cornwall, and the average of the best determined gradients in these districts is probably not the correct average for the present epoch due simply to the original temperature of solidification.

TABLE II.

Duration in years of		Gradient before Glacial period.
Glacial period.	Post-Glacial period.	
20000	10000	One degree in 46·8 feet
40000	10000	,, 44·1 ,,
40000	20000	,, 46·9 ,,
60000	20000	,, 45·9 ,,
80000	20000	,, 45·3 ,,

(2) Estimates of the earth's age founded on Lord Kelvin's solution and conditions, if derived from a temperature gradient in a recently glaciated district, will err by being too great. For instance, if the duration of post-Glacial time be 10,000 years and if that of the Glacial period were four times as great, the normal temperature gradient at the present time would be nearly one degree in 44·1 feet, instead of one degree in 50 feet. The corresponding value of the earth's age would be about 75 million, instead of about 100 million, years.

(3) Lastly, supposing it in our power to make very accurate observations of underground temperature, it would be possible, by comparing gradients in glaciated districts in the two hemispheres, to determine roughly whether their respective Glacial periods ended nearly simultaneously or at widely different epochs; and thus to obtain some suggestion of the direction, cosmical or terrestrial, in which to look for the origin of the glacial cold.

V.—On the Chalk of the London Basin in regard to Water Supply.

By W. Whitaker, B.A., F.R.S., F.G.S., Assoc. Inst. C.E.

[Reprinted from the Report of the Royal Commission on Metropolitan Water Supply, Appendices to Minutes of Evidence, pp. 433–5 (1893).]

1. Thickness of the Chalk.

OVER the area in question the total thickness of the Chalk (where it can be measured from top to bottom, or nearly so) varies from 623 feet at Streatham to 1146 feet at Norwich, and in the latter case the topmost beds are absent; so that the full thickness,

in some parts of Norfolk, may be 1200 feet or more. This figure, however, is exceptional, the nearest to it recorded being 890 at Harwich [this should probably be 895, the Glauconitic basal bed having been called Upper Greensand]. At East Horsley, in Surrey, a thickness of 817 feet has been proved, but generally speaking we find less, and over a large area 700 feet is not reached. It seems that the Chalk thins toward London (where 650 feet may be taken as about the usual thickness), not only from north and south but also from east and west. [It may be well to add the thicknesses proved by other borings outside London. These are as follows, where the borings have passed through the whole formation, from the overlying Tertiary beds to the underlying Upper Greensand or Gault:—Loughton 651 feet, Richmond 670, Cheshunt 681, Chatham and the neighbourhood 680–684, and Winkfield, near Windsor, 725. In sundry places where the topmost beds are missing we have the following:—Bushey (Herts) 686, Chartham (Kent) 735, and Coombs (Suffolk) 817, which last-mentioned should probably be over 820, for the same reason as given above, for increase at Harwich.]

2. Divisions of the Chalk.

Of old the Chalk was usually divided simply into Upper and Lower, the former being also known as the Chalk with flints [but the occurrence of flints is not always a safe guide]. Of late years, however, a further division has been made by the splitting up of the old Lower Chalk into Middle and Lower. This last, too, is often again divisible, by the separation of its lower member, the Chalk Marl.

Besides these divisions certain zones have been marked off, but chiefly from the more or less common occurrence of certain fossils in them. These zones, however, need not be noticed now; it is enough to note the divisions that are based on structural features and that occur over large tracts.

The *Upper Chalk* of this great district is marked by the general prevalence of flints, mostly as separate nodules, in layers or scattered; but also in continuous sheets. Often, however, we find a considerable thickness in which flints are very rare. No other peculiar structure is notable in this division; the beds are sometimes thick, sometimes comparatively thin, and usually there are more or less marked planes of jointing.

The *Middle Chalk* is, on the whole, without flints; but sometimes a few flints are found in it toward the middle part. It is thick-bedded and jointed. Its top is generally shown by the presence of hard beds, consisting of cream-coloured crystalline limestone where well developed, and with green-coated nodules. This is known as the Chalk Rock, and is often to be seen in Hants, Wilts, Berks, Oxon, Bucks, Beds, and Herts. It is less clear in Essex and Cambridgeshire, has not been noticed in Suffolk, and but one section is known in Norfolk. In Surrey and Kent, though no bed with the distinct lithological character of the Chalk Rock has been seen, yet microscopic investigation proves that it is represented. The bottom

of the Middle Chalk, again, is defined by beds of hard chalk, known as the Melbourn Rock, of a more distinctly bedded character than the chalk above, and having usually a nodular structure and sometimes a brecciated one. As far as our work in dividing the Chalk has yet gone, this bed is proved to be of constant occurrence, though varying in its amount of distinctness. [Since this paper was written, it has been found that the Melbourn Rock is absent in the far west, in Devonshire.]

The *Lower Chalk* is distinguishable from the Middle Chalk by the much thinner character of its beds, the planes of bedding being sometimes emphasized by marly partings [especially in the Belemnite Marl, which next underlies the Melbourn Rock] It is often slightly clayey, and the Chalk Marl is sometimes particularly so. This lowest member is in places separated fairly sharply from the rest by a third hard bed, the Totternhoe Stone, a gritty, brownish-grey, firm bed. There are no flints in the Lower Chalk of the London Basin.

Any attempt to divide the Chalk into White Chalk and Grey Chalk is, I think, inconvenient. Colour is generally a very bad guide in making stratigraphic divisions, and the greyness of chalk sometimes depends on its dampness rather than on any inherent colouring. Moreover, the term Grey Chalk has been used in so varying a way as to have become confusing. Sometimes it has been limited to the Chalk Marl; sometimes it has been used for the whole of the Lower Chalk; now Prof. Dawkins seems inclined to include the Middle Chalk also under this head. In the first two cases the term is simply a synonym, and therefore useless; in the last it joins together two divisions which are distinctly separable, and is therefore worse than useless.

As the divisions, Upper, Middle, and Lower Chalk, have been mapped, more or less completely, by the Geological Survey, in the London Basin, from Hants and Wilts through Berks, Oxon, Bucks, Beds, Herts, Essex, Cambridgeshire, and Norfolk (partly), and also outside the London Basin, in parts of [Devon], Somerset, Dorset, Wilts, and Hants, and all through Sussex, I think that they may be accepted as practical.

3. Flow of Water in the Chalk.

The passage of water through the Chalk occurs in three ways.

(1) Through the pores of the rock. As these are very small, sometimes indeed practically closed, this is of course a slow process, and results, in underground work, merely in the weeping or oozing out of water from the cut surfaces. Nevertheless, there may be exceptional cases where, from greater openness in the rock, a greater quantity of water may pass through.

(2) Along planes of bedding, or the originally horizontal layers of which the Chalk is built up. Sometimes the passage of water along these planes is insignificant, but sometimes it is very marked, and this naturally occurs where the bedding is more definite or somewhat open, as along layers of flints. Where, too, marly layers occur, their more impermeable character, as compared with the rest

of the Chalk, is likely to cause a flow of water; so also may an alternation of more open with more compact rock.

(3) The chief means of water-communication through the Chalk is by the more or less vertical fissures that occur, either along planes of jointing (structural planes produced after solidification, and not connected with bedding), or more rarely, along faults or cracks. Joint-planes are universal in the Chalk and mostly very plentiful, though often they are so closed as to allow the passage of but little water. They can readily be seen in pits, and it is generally found that the chief ones have a more or less definite direction over large tracts, a good example of this being given by the highest Chalk of East Kent, in which the chief joint-planes run in a north-westerly and south-easterly direction, as is the case also near Grays, in Essex. The planes of master-jointing are connected by other planes running across them.

In underground work it is sometimes found that the chalk is affected by a great number of these joint-planes, none being open, and each giving passage to only a small quantity of water, though the result of the whole may be very satisfactory. In other cases a long extent of chalk may be passed through with but very slight result as regards water-yielding fissures, and then a large fissure may be suddenly struck and a great quantity of water found.

It is very likely that in many of the swallow-holes (which will be described in a separate paper) the water enters the Chalk at a fissure or crack, in fact along a plane of weakness.

The above remarks refer to the ways in which water gets downward into the body of the Chalk; it may be well to consider its more horizontal course toward the parts where it finds an outflow in springs.

The fissures alluded to naturally form a means of communication, not only more or less vertically downward, but also lengthwise along their course, from the higher to the lower grounds.

The same, too, would be the case with planes of bedding, where they are fairly marked; but it has occurred to me that sometimes too much may have been credited to bedding-planes in this matter. From the fact that the Chalk dips inward, from its generally lofty escarpment to the lower ground formed by the margin of the Tertiary Basin, and that therefore the dip-slopes of the Chalk form by far the broadest parts of the outcrop, it follows that the water in the Chalk would have a general flow in that direction, or inward to the central part of the Basin. This flow in the direction of the dip is, of course, subject to lateral change, where valleys are cut through the Chalk to some depth, and an outlet for water is made.

When, however, we examine other smaller tracts, where the slope of the surface does not go with the dip of the beds, we find a wholly different state of things. The one constant and recognized exception is the face of the great escarpment, that is the outward and generally abrupt slope of the Chalk; the crest of the escarpment forms a watershed, and the underground water flows outward, in the direction of the slope, which is the reverse of the dip. The many springs that occur at or near the foot of the escarpment are proof of this flow.

In the more exceptional cases where the escarpment is not a simple ridge, but a double one, the two ranges some way apart (as near Luton), there is a greater area subject to this flow against the dip, and still more so is this the case where, instead of the usual bold abrupt ridge, we have a more gentle and therefore a broader slope (as in Norfolk); so that over a tract some miles in width the plane of saturation will slope in a direction opposite to the dip.

Another marked exception is given by a range of sea-cliffs, where this runs along the dip, or at no very great angle with the direction of the dip, as on the coast near Dover, in which case the water finds a ready outlet along the free margin formed by the cliffs, and takes a shorter course more or less across the dip, instead of a longer one following the dip.

In short, the underground flow depends generally on the surface-contour; the water can generally flow across the bedding when it can more speedily reach an outlet by such a course.

4. Relation of the Structure and Position of the Chalk to Water Supply.

Turning now to the bearing of the different divisions and of the various structures of the Chalk on the question of water supply, we find, in the first place, that the greater part of the area of outcrop is formed by the Upper Chalk, which, rising up from beneath the Tertiary beds, reaches right on to the top of the great escarpment, or bounding-ridge, except where cut through by valleys, which lay open lower beds. Where the escarpment is all in one, as is commonly the case, and is abrupt, it follows that the outcrop of the lower divisions is narrow, very narrow as compared with the broad tract of Upper Chalk inward from the escarpment. This division, too, is the thickest of the three.

The Upper Chalk, therefore, must be the great gathering ground for water; and, as the water sinks downward to low levels, to the boundary of the Tertiary beds, so that the plane of saturation comes near to, and sometimes up to, the surface of the ground, it follows that, as a general rule, this division must be the great water-yielding one, and that works for vast supplies, over most of the area in question, must be established near the outcrop from beneath the Tertiary beds, where the plane of saturation, though near the surface, is at a low level, so that underground water tends to flow down to such works and to replace that pumped out from them.

It seems to me that the place taken by the Upper Chalk as the great water-bearing bed may, perhaps, be owing to its favourable *position* rather than to its structure. There is nothing, as a rule, to prevent water sinking down through the Upper into the Middle Chalk, and we find that at high levels, toward the escarpment, the former is dry, its base being above the plane of saturation, which sometimes may be very far down in the latter. In its frequent layers of flints the Upper Chalk may have some advantage over the Middle Chalk, in regard to the flow of water inward from the higher grounds; but probably these also sometimes act as a hindrance

to the downward sinking of water in the rock. In the matter of fissures there may be little difference between the two divisions, and the Middle Chalk suffers from having mostly a far greater mass of other beds over it, which would tend to close its fissures.

The Chalk Rock, which forms the top surface of the Middle Chalk, where well developed, is favourable to the passage of water along it, being usually markedly jointed, and so, *when below the level of saturation,* is often a water-yielding bed; but when above that level, there is nothing to stop its being dry.

Where the escarpment forms a long gentle slope, as in Cambridgeshire and Norfolk, the outcrop of the Middle Chalk is fairly broad, and much water must sink through the rock and find its way out in the springs on the westward slope.

Where, too, the outcrop of the Middle Chalk, though usually much less in width, is cut back by valleys that breach the upper part of the great escarpment, as is the case with the valleys of the Lea (in Beds), of the Gade and of the Bulbourn (in Herts), of the Misbourn and of the Loudwater (in Bucks), of the Thames itself and of the Lambourn and of the Kennet (in Berks and Wilts), there, again, much water is collected by this division, and many springs issue from it, and feed the Thames by its various tributaries.

The Melbourn Rock, at the base of the Middle Chalk, is sometimes water-bearing, by reason of the bed next below (known as the Belemnite Marl) being mostly of a rather clayey character; so that, under favourable conditions, it may delay the downward passage of water.

The Lower Chalk, being generally more clayey than the upper two divisions, is less free to the passage of water. Moreover, its outcrop is mostly narrow, and it is not laid open along valleys as the Middle Chalk often is. Circumstances, therefore, are clearly against this division in the matter of water.

There are, however, places where, from favourable conditions arising, much water occurs in the Lower Chalk, and three special cases may be noticed, the more so as they have largely to do with the lowest and usually the most clayey part of the division, the Chalk Marl.

(1) In Bucks, Beds, Herts, and Cambridgeshire, there is, at the top of the Chalk Marl, a marked hard bed, of a somewhat gritty character, known as the Totternhoe Stone, which is permeable. As the water sinking into this, either directly or through the overlying chalk, is stopped in its downward course by the more clayey beds below, springs are of frequent occurrence along the line of outcrop of the stone.

(2) In Bucks there is sometimes a hard permeable bed in the Chalk Marl, which also gives rise to springs.

(3) In Western Norfolk the Chalk Marl loses its clayey character, and passes northward into hard jointed chalk. The Lower Chalk as a whole, too, is harder than usual, and more permeable. The result is that powerful springs occur either at the very base of the Chalk or some little way above it, and are probably the best source of local water supply.

A case in point is the supply of Wisbech, together with many villages in the Fens, from a spring that breaks out at the edge of the Fen in the valley of the Nar, at Marham, some seven miles south-eastward of Lynn.

Of fairly large supplies got from beds below the Upper Chalk, in the Basin of the Thames, Luton and the Chiltern Hills Waterworks (near Tring) may be mentioned. Several smaller towns, too, depend on the same beds, along the northern outcrop of the Chalk. On the south the outcrop of the lower beds is narrower, but various wells have been carried through the Upper Chalk to these beds, as in the case of the Kenley and Caterham Waterworks. Maidstone gets a great part of its supply from Lower Chalk springs, and Folkestone gets the whole.

As a general rule there is more or less communication downward through the Upper and Middle into the Lower Chalk, so that the water in the Chalk may usually be treated as a whole. Nevertheless there are cases where, from the occurrence of less permeable beds in the Chalk over certain tracts, there may be independent supplies at various depths, and cases in point have been alluded to above, with regard to the Melbourn Rock being underlain by clayey beds, and to the Totternhoe Stone as a local water-bearing bed. There seem, too, to be cases, even in the midst of the Upper Chalk, where, from the occurrence of more compact beds at certain depths, there is some division in the water; and again, where strongly marked joint-planes are of rare occurrence, but fairly open where they occur, we may have lateral division in the water flowing through the Chalk, and this water may therefore take the form of more or less defined and separate underground streams.

In conclusion, I would point out that though there are *general* rules as to water in the Chalk, these must not be taken as *universal*: we must be ready for exceptions, and sometimes for great ones. To give what may be called a political illustration—whilst there are imperial laws regulating the conduct of water in the Chalk kingdom, yet the various provinces of which that kingdom is composed need special legislation of their own: rules that hold in one province may not accord with the manners and customs of another [and the same holds for still smaller districts]. In short, it is a case not only for Imperial but also for Local Government.

NOTICES OF MEMOIRS.

Geological Society of South Africa.

I.—Inaugural Address by the President, Dr. Hugh Exton, F.G.S.

After some congratulatory remarks on the installation and progress of the Society, Dr. Exton referred to the interesting studies to be made in various branches of Natural Science, including particularly Anthropology and Geology. The pioneers of Geology

in Africa, Mr. A. G. Bain, Mr. Wylie, and Mr. Stow, had opened the way to the solving of many interesting problems, such as the origin of the great bed of breccia crossing the continent, known as the Ecca, or Dwyka Conglomerate, and the formation of the great valleys dividing the wide areas of Karoo strata, some of the latter comprising rich Diamond-fields and valuable Coal-fields. Still further, the consideration of the nature and origin of the backbone of the Cape country and the Transvaal, whether granitic or quartzitic, often highly auriferous, offer grand opportunities for geological explanation and discussion. Nothing in the known world approaches the banket reef of Witwatersrand, either in the area of country through which it extends, or the average uniformity in its mineral richness. If we consider that within the past twelve months there has been an output of gold to the value of seven millions sterling, and when we see from the working of new companies, and from the more scientific methods of treating the ores, that this output is steadily increasing, we may feel confident that these gold-fields form a centre to which the whole civilized world is looking with profound admiration and eager expectation.

"But there is another factor in connection with the gold-mining industry which we accept too readily as a matter of course, and that is the coal supply, not considering the utter impossibility of working the mines without the steam-power generated by coal; since the supply of wood for fuel (which was used in the early days of the Rand) not only failed to keep pace with the growing demand, but by its great expense stopped the development of the poorer mines, and threatened to render the cost of production in the richer mines greater than the value of the gold obtained. So far back as 1854 Dr. Sutherland directed attention to the existence of coal in Natal. Mr. Dunn's observations, made on behalf of the Colonial Government, were published in 1879, the chief results of which consisted in establishing the relation of the Ecca beds and Karoo series to the coal-seams of the Cape Colony. Indeed, up to 1883, so little had been done by way of proving the existence of workable coal-seams that Professor Green, in his Report to the Colonial Government, recommended that 'suitable rewards should be offered for the discovery of coal as likely to be attended with useful results.' Active search has resulted in discovering the existence of coal at various points over a wide extent of country. It is, of course, of the highest importance that coal should be free-burning and have the property of giving out heat; and these qualities are acknowledged to have been deficient in the coal obtained from seams first opened out in the colony. Professor Rupert Jones has summarized the reports of Mr. Dunn, Mr. North, Professor A. H. Green, and others, who all agree in attributing the deposits of coal to vegetable matter subsiding in fresh-water lakes. The absence of marine fossils was also noted by each observer. Subsequent flooding of such lakes by currents bearing sand and mud would account for the shales with which the coal is so plentifully interstratified. Evidence of the coal having been formed from drift vegetation lies in the

absence of an underclay beneath the coal-seams, such as is generally found below European and American coal-beds, and in which the roots of *Stigmariæ* and other plants are to be found. According to the observations of Mr. Draper, the coal now being worked at Vereeniging, by Messrs. Lewis and Marks, has a soft layer of whitish clay underlying the coal; but, as if to prove the futility of applying general theory to each particular case in South African geology, this underlying clay is apparently a decomposed sheet of dolerite, which had intruded below the coal, and, in many parts, penetrated into it, changing the coal into coke in the neighbourhood of the intrusion, and giving it more the nature of anthracite throughout its area. The endeavour to establish a geological horizon for the coal-beds in South Africa has not hitherto led to much practical result. We have Professor Seeley's recent opinion that coal must not be looked for below the strata containing the remarkable characteristic fossil reptilia. Further, we have Mr. Stow's original deduction that coal would be found from 70 to 150 feet below the silicified tree-stems which he traced over a large area in the north of the Cape Colony, and over a still greater extent in the Free State, and which he termed the forest zone. . . . . Mr. Stow's prediction that the coal would be found to crop out on the north-west of his forest zone has since been abundantly verified at the Vaal River. Nothing like a true Carboniferous system has yet been made out for South Africa; and though ferns, such as *Glossopteris*, have been found, this and some others are looked upon as survivals of Carboniferous plants into Jurassic times."

After some observations on the Coal-beds in the Stormberg and at Lake Nyassa, of the same Karoo series, Dr. Exton concluded with an exhortation to geologists to lose no opportunity in elucidating the many interesting and, indeed, most important problems lying at their feet in the subterranean strata, and before their eyes in the many and varied escarpments of hills and mountains around them.

II.—Glacial Progress. By Captain Marshall Hall, F.C.S., F.G.S.

[From the "Alpine Journal," No. 128, May, 1895.]

SINCE the appointment of the Sub-Committee upon Glacier Observations[1] sufficient time has not yet elapsed for many exact data to have come to hand, with one very brilliant exception—that of New Zealand.

Amongst the explorers of the Southern Alps are men not only mountaineers, but who are also greatly interested in these problems, shrewd observers and efficient officers of the New Zealand Survey. We have the novelty of new excursions, combined with the determination of a series of positions upon which to found future measurements, and all this in mountain ranges till recently scarcely known. The writer will give a summary of this work (with great conciseness, the result of instructions he has received). In the

[1] Geol. Mag., 1895, March Number, p. 144.

absence of key maps, he omits tables, which would not be understood without diagrams of the various localities.

In the year 1892 an exploration was made in the hopes of finding some practicable route for a road from the west coast, Middle Island, New Zealand, across the main range to "The Hermitage." Although this object was not completely attained, materials for a survey of the country were collected, and a map by Mr. C. E. Douglas, explorer, Westland, is published in the Report for 1893 of the New Zealand Surveyor-General, as also a sketch of the geological formation of Copland District and sundry inspiring views of glaciers and peaks. Messrs. Douglas and Cuttance describe the Copland, Lyttle, Strauchon, and Cuttance glaciers amongst other features of the scenery.

In the Report for 1894 Mr. Douglas gives an account of a survey of the Westland Alps, with the assistance of Mr. A. P. Harper, from November, 1893, to April, 1894. On this expedition a satisfactory triangulation was carried up several glaciers. In the case of Franz-Joseph Glacier, points in its neighbourhood and on the ice itself were determined in sufficient number to afford data for estimating its future movements and bulk. This will serve as a typical glacier on the north-west side of the range. Its "snout," 692 feet only above the sea, is but four miles distant from the beach, in lat. 43° 25′ 30″ S., and long. 170° 10′ 58″ E. It has made great winter advance and summer retreat. Débris from lateral ranges have lodged in crevasses of the higher layers of ice, which further down have become the lower layers, the upper ice pushing over the lower,—as is exemplified in photographs to be found in the library of the Alpine Club—showing itself in alternate clear and dirty ice. The upper ice overlaps and breaks off at the terminal face, as also shown in the photographs. Other illustrations and maps will be seen in the Survey Report for 1894, together with a text full of interest. Over one matter the writer has been much exercised. Mr. A. P. Harper gives, at page 77, a table of rates of ice movement during his stay. An average of his figures at different stations gives the daily rate as 154·2 inches. But five other entries are so different, being 5″, 30″, 53″, 23·6″, and 7·28″, that to strike a mean of these figures would evidently mislead. They refer to distances from the sides of the glacier. At page 73 Mr. Douglas makes a statement of considerable interest to students of ancient glaciers. He says: "In valleys containing large glaciers I have always found four tiers of terraces, or old ice-lines, as if there had been four distinct periods. These lines keep a wonderfully regular distance from each other, and their inclination is very uniform, from, say, 4000 feet to 600 feet or 700 feet. . . . . The larger the valley the more gentle the slope."

Mr. T. N. Brodrick, C.E., sent a paper, accompanied with four maps, to the writer. The most important part of Mr. Brodrick's work is a triangulation and survey carried up the Mueller glacier, during which were determined, not only the position of stones on the ice relatively to stations on the huge lateral moraines, but the distance and bearings of many such blocks *from each other*. These

positions were first fixed on March 29, 1889, re-determined November 14, 1890, and again December 3, 1893. The measurements so far showed (what has been found elsewhere) that the sides move more slowly than the centre; that the ice moves more slowly as it approaches the terminal face; that the current is varied by surrounding circumstances, such as bends of the glacier and, probably, the unevenness of its bed.

Its daily rate is not constant; a comparison of the rate for 1889–90 with that for 1890–93 shows a decrease of speed during the latter period. Here we ask, how as to *bulk* in those years? Fluctuations of the terminal faces of the Tasman and Mueller glaciers have been constantly remarked, but changes have not lasted sufficiently long to show if the ice be retreating or advancing. Captain Hutton, F.R.S., President of the New Zealand Alpine Club, states that about 1882 the Mueller glacier reached over the Hooker River to the side of Mount Cook, and sheep were taken over on the ice. Traverses were made of the terminal faces of the Tasman in November, 1890, and of the Mueller in March, 1889, and November, 1890. Mr. Brodrick says that the Hooker River so modifies the contour of the Mueller that the experiment has failed to demonstrate short changes satisfactorily, though these traverses will no doubt be of interest in the future. During a recent period all the Canterbury glaciers appear to have been in retreat, but latterly, and for a number of years, to have been stationary.

There is no evidence of ploughing up the earth and leaving it in ridges, and falling stones are in great measure stopped by lateral moraines, and do not reach the ice. Mr. Brodrick gives tables of rates of motion, and one of the areas of six glaciers, together with that of the sources of supply, as regards *nêvé*.

REVIEWS.

THE SEISMOLOGICAL JOURNAL OF JAPAN. Vol. IV, 1895 (corresponding to vol. xx of the "Transactions of the Seismological Society): Edited by Prof. J. MILNE. pp. xxi + 367.

Contents: A Catalogue of 8331 Earthquakes recorded in Japan between 1885 and 1892, by Prof. John Milne, F.R.S., F.G.S.

MANY of the preceding volumes of this well-known series may have possessed a wider interest, but few, if any, are of greater value, or will more profoundly influence the progress of seismology, than that which is now before us. This evidently is a work which is neither to be "tasted" nor "swallowed," nor even to be "chewed and digested." It is intended for those who make books, not for those who read them. It will be the parent of many papers. For in these somewhat unattractive-looking pages are gathered materials for the harmonic analyzer and facts for the student of terrestrial evolution. Only the labour is required, and that will soon be forthcoming, and we shall know something about the laws which govern the distribution of earthquakes in space and time, at any

rate in the country where they have been most widely and successfully studied.

More than nine-tenths of the volume is occupied with two great catalogues. In the first of these we find the time of occurrence of every earthquake, the extent of *land*-area shaken, and figures which by reference to an index-map give the approximate position of the epicentre and the boundary of the disturbed area; in the second, the lengths of the axes of the disturbed area (thus giving the *total* area), the distance of the epicentre from the shore when submarine, and the seismic district to which the earthquake belongs. The importance of the catalogues will be evident from this brief summary.

In the prefatory pages, Prof. Milne gives an account of some of the first results which have been derived from a study of these materials. Mr. Omori has investigated the distribution of after-shocks in space and time, and has already published an important memoir on the subject (see "Natural Science" for June). On a reduced index-map, the distribution of seismic centres in Japan is clearly exhibited. From this we learn that the central portions of the country are remarkably free from earthquakes, and it is interesting to notice that these are the very districts where mountains and active volcanoes are prevalent. The greater number of earthquakes originate along the eastern coast of the empire, from the face of the steep monoclinal slope which Japan presents towards the Pacific Ocean. The chief anticlinal axis of the island runs N.N.W. and S.S.E., and, from the southerly prolongation of this axis, earthquakes from time to time originate. Again, along lines of great slope (1 in 20 to 1 in 30) earthquakes are frequent, while they are rare where the slope is gentle; and they are numerous in those districts where there are evidences of secular elevation or depression. A map has also been drawn, though not published, which shows the distribution of earthquakes accompanied by sound. Generally, it appears, sound is heard in rocky mountainous districts, while on the alluvial plains it is but rarely observed.

Several of the Japanese earthquakes have been propagated as far as Europe, and have been recorded by horizontal pendulums. Dr. E. von Rebeur-Paschwitz, who has studied the records of these instruments with great ability and success, contributes a list of 301 seismic disturbances recorded by them in different parts of Europe and at Teneriffe. At least seven of these disturbances were due to Japanese earthquakes. The preliminary tremors are estimated to travel with a surface-velocity of 12 km. (about 8 miles) per second, while the larger oscillations follow at a much slower rate. What is the true nature of these remarkable movements, how they travel—whether in straight or in curved lines, whether along the surface or right through the interior of the earth,—these are the new problems which are set before seismologists to solve, and to which their attention is now being closely directed. C. Davison.

REPORTS AND PROCEEDINGS.

Geological Society of London.

June 19th, 1895.—Dr. Henry Woodward, F.R.S., President, in the Chair.

Mr. Whitaker called attention to the two new Survey maps of the Bournemouth district. In the map without Drift it would be observed that the boundary-lines were more detailed. In the map with Drift much new work had been incorporated—in fact, the Drift-lines were wholly new. The work had been done by Mr. C. Reid.

The following communications were read:—

1. "On the Occurrence of Radiolaria in Chalk." By W. Hill, Esq., F.G.S., and A. J. Jukes-Browne, Esq., B.A., F.G.S.[1]

The authors notice the rarity of records of Cretaceous Radiolaria, and allude to those which have been made, including those by Rüst and Sollas. They have recently discovered spherical bodies resembling in form and general appearance certain calcified and partially destroyed radiolarian tests from some of the Barbadian rocks; microscopic examination of these has proved that many of them, at any rate, are Radiolaria. They occur in the nodules of the lower beds of the Melbourn Rock, at Melbourn, Royston, near Hitchin, Leagrave near Luton, Pitstone and Tring, Watlington, the Richmond boring, the lower part of the "Grit Bed" at Dover, Sutton Waldron and Burcombe (Dorset), and in a nodular chalk which may be considered as the equivalent of the Melbourn Rock from Bindon Cliffs, near Axmouth, Devon. Similar organisms have recently been found in the Chalk Marl of Lincolnshire, Yorkshire, and Norfolk, but have not been noticed in any other parts of the Chalk. It is suggested that they occurred in many portions of the Chalk-ooze, but were usually rapidly and completely dissolved, and contributed to that solution of silica which furnished the substance of flint-nodules; and the authors conclude that the preservation of traces of the Radiolaria in the nodules of the Melbourn Rock is due to some specially favourable conditions.

A description of the changes undergone by Barbadian Radiolaria is given to illustrate the instability of radiolarian tests. Here all stages are traceable, from the perfect siliceous test to a structureless ball or disc filled with calcareous matter, or a mere patch of clear crystalline material.

A description of forms recognized in the nodules of the Melbourn Rock is given.

2. "The Crush-Conglomerates of the Isle of Man." By G. W. Lamplugh, Esq., F.G.S.; with an Appendix by W. W. Watts, Esq., M.A., F.G.S. (Communicated by permission of the Director-General of the Geological Survey.)

The Skiddaw Slates of the Isle of Man have everywhere undergone intense shearing, and on the north-west side of the main stratigraphical axis actual disruption of the bedding with the resultant formation of breccia or crush-conglomerate on a large

[1] See also paper by Mr. G. E. Grimes on Radiolarians in the English Chalk, *ante*, p. 345.

scale has taken place. This structure attains its widest development on the north side of the central valley, though it is noted on a more limited scale in a few localities farther south. It is well exposed in the cliffs three miles north of Peel, but the finest sections are in the interior in Sulby Glen, where the structure has a thickness of some hundreds of feet, and runs continuously about north-north-east for five miles, with a probable extension southward for three miles farther, and also eastward for other four miles. It is usually flanked by gritty flags on one side, and by dark clay-slates on the other. It is affected by a strain-slip cleavage later than the brecciation, and several igneous dykes which intersect it are affected by the same cleavage, though not by the brecciation-movement.

East of Sulby Glen the structure extends towards Ramsey, at first in a horizontal spread over a mile in width, and afterwards in a series of comparatively narrow subparallel bands; and it is suggested that we are dealing here with the emergence of a deformed plane. The structure is continued eastward into the hill overlooking Ramsey, beyond which it is cut off by the sea. In this locality, as elsewhere, the sections show the gradual smashing into fragments of highly contorted strata until every trace of the original bedding is lost, and a "crush-conglomerate" with lenticular and partly rounded inclusions is formed.

The rocks described in Mr. Watts' Appendix are grouped in four classes. Firstly, the grits and slates which have been crushed but have not been converted into crush-conglomerates; secondly, the crush-conglomerates themselves, and the fragments which they contain; thirdly, the dykes of decomposed dolerite (greenstone) and fresh later dolerite which penetrate the conglomerate; fourthly, a portion of the crush-conglomerate metamorphosed by these intrusions.

The chief point of interest is brought out by the examination of the fragments in the conglomerate. All stages of crushing may be traced, until the grit-fragments have a structure which is a mere miniature of the crush-conglomerate itself; that is to say, if the crush-conglomerate be regarded as made of "fragments" of hard rocks enclosed in crushed "matrix" of soft rocks, a host of intermediate varieties with varying resistances will occur. Whether any particular one of these varieties shall pass into "fragments" or "matrix" under the crushing will depend upon the amount of the crushing force and upon the association (paragenesis) of the material brought together to be crushed.

3. "The Chalky Clay of the Fenland and its Borders: its Constitution, Origin, Distribution, and Age." By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

The distribution of the Clay (so often termed Chalky *Boulder*-clay) is noticed, and it is stated that it is surrounded on all sides by country occupied by different deposits, being mainly separated from the sea on the east and north-east by sandy and pebbly materials, while on every other side it is clearly and sharply defined. The paucity of foreign stones is noted as compared with natives, and

the similarity of the matrix of the Chalky Clay to the material of the older deposits of the neighbourhood. The author maintains that the contents of the Clay indicate movement of material from west to east in some places, as shown by Jurassic fossils in the East Anglian Chalky Clay, and from east to west in others: in fact, that movement took place in sporadic lines diverging from the Wash and the Fens. He appeals to the amount of disintegration that has taken place to furnish the material for the Clay, the shape of the stones in the Clay, and the distribution of the Clay itself, as evidence against the action of land-ice or icebergs, maintaining that there is no evidence of submergence at the time the Clay was formed; and criticizes the attempts made to explain the formation of the Clay by water produced by the melting of ice.

The author believes that the denudation of the Fen country which produced the great mass of the Chalk Clay with most of its boulders was coincident with and caused by the bending and folding of the Chalk of Eastern England, which took place after the deposition of the Crag beds, and that during the period of folding a great depression was formed round the Wash, into which the water rushed from the North carrying débris and mixing it with clays; this, rushing into what was virtually a *cul-de-sac*, dispersed and scattered its load in all directions.

4. "On the Occurrence of *Spirorbis*-Limestone and thin Coals in the so-called Permian Rocks of Wyre Forest; with considerations as to the Systematic Position of the 'Permians' of Salopian type." By T. Crosbee Cantrill, Esq., B.Sc. Lond. (Communicated by Walcot Gibson, Esq., F.G.S.)

In South Staffordshire a thick series of red rocks—the so-called Lower Permian—overlies the ordinary yellow and gray Coal-measures, and underlies the Triassic rocks. They consist of sandstones, marls, calcareous conglomerates, and breccias, having a general red or purplish-red colour.

Since Jukes' work was published, fresh sinkings have shown that these red rocks must be regarded as of Upper Coal-measure age, because their included fossils have an Upper Coal-measure *facies*. The rocks contain bands of limestone characterized by the presence of *Spirorbis pusillus*: those parts of the series which have not yielded Coal-measure fossils are apparently similar lithologically to those which have yielded them; there is no stratigraphical break between the fossiliferous and unfossiliferous parts of the red series, and the only marked breaks are at the base and summit of the series, the break at the base being locally great but elsewhere practically imperceptible.

The evidence furnished by the deposits of the Forest of Wyre (= Enville) district also leads the author to regard the red rocks associated with *Spirorbis*-limestone and coals as Upper Coal-measures, exhibiting a gradual passing away of Coal-measure conditions and the in-coming of those of New Red Sandstone times; and these passage-beds must be regarded as much nearer the Coal-measure than the Permo-Triassic end of the transitional period.

So far as our present knowledge goes, the so-called Permian rocks of Anglesey, Denbighshire, Lebotwood, Shrewsbury, Coalbrookdale, Wyre Forest, South Staffordshire, Warwickshire, Leicestershire, and North Staffordshire are all essentially similar; and observers are urged to look out for Coal-seams, plant-remains, and *Spirorbis*-limestones.

[The next Meeting of the Society will be held on Wednesday, November 6th, 1895.]

CORRESPONDENCE.

NATURE AND ART.

Sir,—It has been suggested (and the belief is held by many) that the asserted flint implements of the Chalk Plateau of Kent may be divided into two groups—one (A) in which the presumed work is referred to the *agency of nature*, and is therefore non-existent so far as human agency is concerned; and the other (B) of which the work is admitted, but is asserted to be that of Palæolithic Man, and not therefore belonging to a still older race of Man, to which on geological grounds I have assigned them.

B.—It may be admitted as not improbable that some of the more highly finished implements found on the plateau (if not made by the more skilful workman of Eolithic Man) may have been the work of Palæolithic Man. But the mere circumstance of their being found on the same surface with the plateau implements proves nothing, as flint implements of undoubted Neolithic age are likewise found frequently associated with the older plateau as well as with Palæolithic implements on the same surfaces. Further, in a pit dug for the Committee of the British Association by Mr. Harrison, several plateau implements were found in a bed of clay and flints at the depth of six feet from the surface, but no implements of the Palæolithic type were met with there. We may presume, therefore, that the association of implements of different ages on the surface is accidental, and does not always prove that they are contemporaneous.

With respect to the other question (A), I am at forced issue with those who would ascribe any of the forms of the rude plateau flints to any natural agency, such as, for example, as has been suggested in explanation of one form—that of the action of the waves on the shore, or of river-action; and there can be no other. Angular pieces of limestone put into a mill come out rounded marbles. In like manner the sharp angular fragments of flint exposed to the beating of the waves soon lose their sharp angles and become gradually more and more worn, until eventually they are transformed into rounded shingle, such as that which may be seen on Blackheath Common and at Bickley station. No other result is possible. This fact must have been brought home to many who have lounged pleasantly on the sea-beach at Brighton or Dover, but who would not find a bank of plateau flints with their sharp angles and many points, so well fitted for that purpose. Had it been possible for sea- or river-action to have produced such forms as those I have

figured in plates v to ix of "Collected Papers," they should be found in all such shingle of whatsoever age. None are forthcoming.

If the waves had possessed that power, we should not need to go beyond our beach-girt southern shores or our numerous river-gravels. Challenged long ago to produce an implement of the true plateau type from a recent beach, the only specimen that has been put forward as such is one from the beach at Aldborough. I have examined this specimen carefully. It is a *naturally* split pebble of the Westleton pebble beds, of which there are millions, *entire* and *split*, on the adjacent shore. These, in fact, constitute a very large proportion of the beach itself, and those of them which happened to be split have had their edges chipped and blunted by the pounding they have undergone on the shore, so that to that extent they resemble the one plateau specimen figured, fig. 4, pl. ii, in the work before mentioned, with the essential exception that whereas the latter often retain the sharp edges which adapted them for scraping, in the former the edges, which are also worn and blunt, were never suited for that purpose. It is, in fact, one of those natural flints which *simulate* in general outline a worked flint, and in this case I am willing to admit that the simulation of this one simple form of the plateau flint is very good and very deceptive. Scores of such natural forms, imitating even the well-defined shapes of the lance-head Palæolithic implements, have been found in gravel beds.

However, to put the matter to another test, I again repeat the former challenge, and am ready to exchange the two volumes of my "Geology" with any young (or old) dissentient, for half a dozen shore flints (not derived) of any of the plateau types figured in the five plates above named.

I have noticed with regret that in discussing the minor points, the essential and important fact of the plateau implements being possibly the work of the earliest known members of the human stock, has been too much overlooked. While anthropologists have sought for and described the stone implements of modern savages, from the poles to the equator, and speculated on their uses, they have with a few rare exceptions shown an unaccountable indifference, not only in the plateau specimens themselves as specimens, but also an unwillingness to give the subject that attention which alone could settle the question. Here is a problem of high importance with respect to the habits, mode of life, and characters of primitive man, as exhibited in a wonderful profusion of their rude tools, and which is nevertheless neglected and rejected, not from personal investigation, but on an assumed impossibility and by an abnegation of personal responsibility. Surely the subject is deserving of further investigation. I wait without anxiety the results of my challenge.

SHOREHAM, KENT, *July* 15, 1895. JOSEPH PRESTWICH.

DR. CALLAWAY AND METASOMATOSIS.

SIR,—The letter of the Rev. J. F. Blake in last month's GEOLOGICAL MAGAZINE does not call for lengthened comment.

He makes it quite clear that in his criticisms of my work at Malvern he had employed the theological, rather than the scientific, method. I had often said that a gradation between two kinds of rock at Malvern proved the derivation of the one from the other; but then I had previously explained, by an accumulation of details, what I meant by a Malvern gradation. Mr. Blake forgot the details, and suggested that I believe any kind of gradation would prove my case. It is possible to involve any author in apparently contrary opinions, if his sentences may thus be detached from their context.

But Mr. Blake goes on to admit that he does not really suppose me to hold the opinion that "if one rock passes into another, one of them is derived from the other," but he does think I argue on the basis of the following: "The character of the stages by which one rock passes into another in the field *may* suffice of itself to prove that one of them is derived from the other." This is quite another thing, and should have been said at first.

Mr. Blake's use of my illustration of the beef cooking before the fire is rather misleading. The roasting meat undergoes a change, and so do the Malvern rocks during metamorphism. The cook is able to observe the change, and so can the geologist at Malvern. These two points exhaust the analogy as I limited it, and to expand it to cover a special theory of metamorphism is to commit a fallacy in logic.

My critic—perhaps I may say "critics," for Mr. Blake follows in the wake of others—appears to think that no amount of microscopic or outdoor evidence will suffice to prove the conversion of a diorite into a quartzose gneiss (not "quartz-schist," as Mr. Blake inaccurately writes). If this opinion be right, we are shut up in eternal darkness on this question, for no other proof is available. Surely, our ignorance of the chemistry of high temperatures and great pressures ought not to be erected into an insuperable bar against the reception of good field-evidence. Are my critics going to wait until earth-forces can be introduced into the laboratory?

WELLINGTON, SALOP. C. CALLAWAY.

BOULDERS IN A COPROLITE BED AT STANBBIDGE.

SIR,—A bed of "coprolites" is now being worked near Stanbridge, in South Bedfordshire, at a spot about half a mile west of the church, just in the angle formed by the roads from Leighton Buzzard and Billington. The bed resembles in many respects the Cambridge Greensand; its thickness varies within short distances, the maximum being about one foot. Fossils are not abundant, but those found belong to species which occur in the Cambridge Greensand. Mr. Jukes-Browne made a fresh survey of the district some ten or eleven years ago, and determined that the bed, which was at that time exposed in some other pits in the neighbourhood, occurred at the base of the Upper Gault, which in this district is very marly, yielding over 50 per cent. of carbonate of lime.

Two boulders of quartzite have recently been found in the

Stanbridge pit, and were presented to the Woodwardian Museum by Mr. H. Coningsby; they are angular blocks, 10 × 6 × 6 and 7 × 7 × 6 inches in size, and weigh 16 lbs. and 13 lbs. respectively. Attached to their surface are numerous specimens of *Plicatula sigillina*, Woodw., and *Spondylus striatus* (Sow.), and small concretions of iron pyrites. The quartzite is rather coarse-grained, and was originally a quartz-conglomerate. H. Woods.

NOTE ON THE ERRATIC BLOCKS OF POLARIS BAY AND OTHER LOCALITIES IN NORTH GREENLAND.

Sir,—I should like to place upon record a statement I ought to have published years ago, but which at the time the subject was fresh I thought of too little moment to record. Since then I notice that what I believe to be an erroneous deduction, has been cited by authors of eminence as a fact, without any qualification. The late Dr. Emil Bessels, when in Hall Land, North Greenland, with the "Polaris" Expedition, noticed that the land in the vicinity of the "Polaris" winter quarters was strewn up to elevations of over 1,000 feet with erratic ice-borne boulders, entirely distinct in character from the rocks *in situ*. Bessels, who was a man of high attainments, and a good observer, misled, I believe, by many of these erratics having a superficial resemblance in composition to rocks found in South Greenland, made the sweeping assertion[1] that these erratics came from South Greenland; and that the current and ice-drift in Smith Sound and Robeson Channel, at a former period when these erratics were dispersed, had been from south to north, and not from north to south as is the case to-day. When I sojourned in Grinnell Land, during 1875–76, I was aware of Bessels' opinion, and made many observations on the boulders we met with, and on their distribution. I found them as Bessels described, even on the higher altitudes uncovered by snow, notably on Dean Mount, near the winter quarters of the "Alert," in 82° 27′ N., at an altitude of 1,200 feet. I observed, however, what Bessels seems to have overlooked, that boulders of the same character were strewn over hillsides and in the valleys down to the present sea-level, and that on some ancient sea-beach at a hundred feet of altitude the stranded boulders were lying under precisely the same conditions as the boulders that now rest on the seashore of to-day, and which have been recently stranded. Grinnell Land is an area of very rapid elevation, and it is only reasonable to argue that the agent that strands the ice-worn boulder to-day on the fore-shore of Grinnell Land is the same as placed the boulders at an altitude of 1,200 feet when that point stood at sea-level. The agent that grounds the erratic of to-day is the ice-raft of the palæocrystic sea, and Bessels was certainly mistaken when he ascribed the origin of these rocks to South Greenland, and to clench his argument had to invoke a change in oceanic circulation to account for the presence of the boulders on the shores of the Polar Ocean. Bessels made a strong point that

[1] Bull. de la Soc. Geog. Paris, p. 298, March, 1875. U.S. Naval Report, 1873, p. 548. Arctic Manual, p. 553, 1875.

one of the most marked characteristics of some of these boulders was their containing large garnets, similar to those in a rock found in the neighbourhood of Fiskernäs, in South Greenland. It is perfectly correct that some of the boulders I met with in Grinnell Land were of garnetiferous gneiss, and the difference between them and the blue Silurian limestone or dark Azoic slate, the rock *in situ*, on which they were lying, could hardly fail to attract attention. From Lady Franklin Bay, on the west side of Robeson Channel, as far north as Cape Joseph Henry, I did not meet with this garnetiferous gneiss as a rock of the country. It does not occur as such in Hall Land, neither do I think it can be found *in situ* along the lands visited by Aldrich in his journey along the north shore of Grant Land, nor on the northern shores of Greenland traversed by Beaumont, Lockwood, Brainard, Peary, and Astrup, for this rock is of such a striking character that such intelligent observers as I have mentioned could not well have passed over it without remarking its garnetiferous structure. There was a fine example of an ice-rounded boulder of this remarkable rock lying stranded a little above high-water line, not far from the "Alert's" winter quarters, in 82° 27′ N. Fortunately I brought away with me fragments of this boulder, which are now in the British Museum; I am informed that the fragment is a "coarse-grained aggregate of large garnets, orthoclase, cordierite, fibrolite, and quartz, with a little biotite. A peculiar feature of the rock is the enclosure of rounded quartz crystals in the felspar. It is probably a garnetiferous cordierite-fibrolite gneiss, but the fragment is too small to show the foliation."

The conclusion I arrive at is, that this erratic boulder and its fellows, scattered over the shores of Grinnell Land and North Greenland, cannot by any possibility have been derived from South Greenland, and floated up through Davis Strait, Baffins Bay, Smith Sound, and Robeson Channel, to the Polar Sea. Such a supposition is as much at variance with fact, as to cast a bladder into the sea at Cape Clear, and assure us that it reached the West Indies; and I cannot understand how a man of Bessels' ability could formulate such a theory. It is far more reasonable to presume that these erratics are derived from some land within the unknown region of the Polar area. If so, the land that produces them must support glaciers, for these ice-worn boulders have passed through the mill of a glacier, to give them their present shape and wear. There can be little doubt that the drift-wood stranded on the shores of the Polar Ocean, along the coast of Grinnell Land, is derived from the great rivers of Siberia, and the same drift and current that transports it is equally capable of drifting ice-borne erratics from unknown Polar lands.

H. W. Feilden, Colonel R.A.

West House, Wells, Norfolk.

EXPANSION THEORY OF MOUNTAIN EVOLUTION.

Sir,—Mr. Davison, in a "Second Note on the Expansion Theory of Mountain Evolution," quoting Prof. Le Conte, restates what he thinks to be a fundamental objection to the expansion theory.

He does not, however, allude to, and therefore possibly has never taken the trouble to read, the several direct replies I have given to this very objection when made by people who had failed completely to understand what my theory, as originally published in 1886, really is. One such complete refutation is contained in a paper in this MAGAZINE in May, 1894, entitled, "On the Result of Unsymmetrical Cooling and Redistribution of Temperature in a Shrinking Globe as applied to the Origin of Mountain Ranges," in which I have dealt broadly with the whole subject, and have shown that the effect of sedimentation is to check the Earth's cooling in the area in which the sediments are being laid down. The form in which the case is stated, both by Le Conte and Davison, shows an entire misapprehension of the whole problem. It is not that the sediments abstract heat from the portion of the Earth's crust upon which they lie or with which they are in contact, but that they simply prevent the outflow and consequent loss of that heat from the nucleus which would otherwise be dissipated into space, and this heat thus retained expands them. I have shown in the paper referred to that it is to the *redistribution* of heat in the crust, and the consequent alteration of stresses and strains, that we are to look for the effective cause of mountain folding and upheaval. To say, therefore, that the "*increased heat must be taken from somewhere else*" is an obvious truism which conceals the implied fallacy that sedimentation at a certain point will cause a greater loss of heat at that point than at the surrounding points whereon no sedimentation has taken place, and this is not true—it is exactly the reverse—less heat will be lost at that point.

Mr. Davison, thinking to strengthen his objections to the expansion theory, now quotes Herschel and Lebour to show that wet rock is a better conductor than dry rock. This was well known to me, through their admirable researches, before I penned a line of the "Origin of Mountain Ranges." Doubtless superficial layers of wet sediment may be better conductors of heat than the average earth-crust, but consolidated sediments miles thick are not likely to be in the moist condition common to surface-rocks. Herschel and Lebour's investigation, so far from supporting Mr. Davison's contentions, are on the whole distinctly in favour of my views. Indeed, I cannot do better than reproduce here some of their extremely acute observations on the results of their experiments, which have so far not received from geologists and physicists the attention they deserve—"In the first place, it seems to be proved by our experiments that the conducting-power of different rocks varies strictly according to their lithological character. Very crystalline rocks, such as granite and serpentine and statuary marble, allowed heat to pass rapidly through them; slate-plates, with their uncrystalline compact structure, had a still higher degree of conductivity. The crystalline nature of a rock alone is not, therefore, the lithological test of its conductivity. The lowest powers of conductivity were found to belong, among the specimens experimented on, to shale; the black shale, which was lower than the grey, is softer and more

argillaceous than it, the grey shale having a considerable admixture of arenaceous matter and mica. The difference, however, between these two was so slight that, in the present preliminary researches, when much must be allowed to error, it may be left out of consideration altogether. It would appear, then, from these facts, that a certain compactness, accompanied by cleavage, is favourable to the passage of heat through rocks; and if it be admitted that what is true for small thicknesses is also true for great ones, we may be justified in supposing that the vast masses of clay-slate, and perhaps to a still greater extent their more metamorphosed and crystallized schists (which we know to extend to great depths), are so many points of weakness which must have their influence in the secular cooling of the earth. On the other hand, points of resistance may be assumed to exist, and to be formed by the great sedimentary accumulations of shale, and probably also of clay and other argillaceous unaltered rocks. In a column, therefore, composed in part of cleaved clay-slate and in part of shale, the easy passage of the internal heat outward through the first would be checked through the other in the ratio, roughly speaking, of five to eight. This becomes a stupendous difference when we apply it to the thicknesses we are acquainted with. If we imagine a thick covering of shale or clay or some other rock with a very *low* conductivity which has arrested in its course the heat passing up to it through underlying rocks with a high degree of conductivity—if we imagine such a surface-covering removed (as we know that they frequently have been) by denudation, it is evident that the equilibrium of the heat-resisting covering of the earth will be altered, not only at this particular spot but also wherever the material removed is being redeposited." [1]

It is obvious that the drying and consolidation of sediment goes on concurrently with its increasing depth, and it is probable that the piles of horizontal sediment miles thick which are laid down as the materials out of which future mountain-ranges are built possess a lower conductivity than the substratum of the crust, largely crystalline, upon which they rest.

But whether these sediments are better or worse conductors of heat than the average crust does not in the least affect the principle of their action as intercepters and accumulators of the heat outflowing from the nucleus; and if Mr. Davison had correctly apprehended this fact, instead of confusing the issue by the repetition of a fundamental misstatement, we might not have been favoured with No. 2 note on the "Expansion Theory of Mountain Evolution."

PARK CORNER,
BLUNDELLSANDS, LIVERPOOL.

T. MELLARD READE, C.E., F.G.S.

[1] British Assoc. Report, 1873, Appendix p. 226: "Notes on the Conducting-power of certain Rocks," by Herschel and Lebour.

OBITUARY.

VALENTINE BALL, C.B., M.A., AND LL.D. (DUBLIN), F.R.S., F.G.S., M.R.I.A.

BORN JULY 14TH, 1843. DIED JUNE 15TH, 1895.

FEW men of science were more widely known or more cordially esteemed than Dr. Valentine Ball, the distinguished Director of the Dublin Museum of Science and Art, whose premature loss we have to deplore.

Valentine Ball was born in Dublin July 14th, 1843, and was the second son of the well-known Naturalist, Robert Ball, LL.D., who died in 1857. He was educated at Dr. Brindley's, Chester, subsequently at Dr. Henry's and Dr. Benson's private schools in Dublin, and at Trinity College, Dublin. He graduated in the University of Dublin, B.A., 1864; M.A., 1872; LL.D. (*honoris causâ*), 1889. He was elected a Fellow of the Geological Society of London in 1874; Fellow of the Calcutta University (*honoris causâ*) in 1875, and Fellow of the Royal Society of London in 1882. He was elected President of the Royal Geological Society of Ireland in 1882. He was appointed Clerk in the Receiver Master's Office, Dublin, 1860–64; and joined the Staff of the Geological Survey of India in 1864, and served till 1881. On his return to Ireland he was appointed Professor of Geology and Mineralogy in the University of Dublin, and held office from 1881 to 1883, when he became Director of the Science and Art Museum in Dublin, which office he held until his death. Dr. Ball was also Honorary Secretary of the Royal Zoological Society of Ireland, and a Member of the Council of the Alexandra (Ladies) College, and of that of the Royal Irish Academy.

His published works are—"Jungle Life in India, or the Journeys and Journals of an Indian Geologist," 1880; "The Diamonds, Coal, and Gold of India," 1881; "The Economic Geology of India," 1881; an English Translation of "Travernier's Travels in India," with Notes and Appendices, etc., 1889. Besides numerous contributions to learned societies, he published several Memoirs on the Geology of extensive tracts in India, and accounts of his visits to and explorations in Afghanistan and Beluchistan, the Andaman and Nicobar Islands, the Himalayas, etc. As a collateral result of his explorations in the wild and then little-known central regions of the Peninsula of India, where he first discovered several coal-fields, he was enabled to suggest to the Government the most desirable line of route for a direct railway between Calcutta and Bombay. This route has now been adopted, after several years spent in surveys of the various alternative routes. Several of his more important recent contributions to societies are upon the "Identification of the Animals, Plants, and Minerals of India which were known to the Ancients." In the year 1884 he presented a Report to the Science and Art Department on the Museums of America; it was subsequently published in the Department's Annual Report. He contributed three articles to the GEOLOGICAL MAGAZINE on the "Volcanoes of the Bay

of Bengal," 1879, p. 16, Pl. I; 1888, p. 404; and 1893, p. 289, Pl. XIII; and "On the Mode of Occurrence of Precious Stones in India," 1884, p. 516; "On Eroded Agate Pebbles," 1888, p. 231; "On the Transport of Granite found in the Carboniferous Limestone, Dublin," 1888, p. 232. He filled the office of President at the late meeting of the Museums Association in Dublin in 1894. With most of the scientific societies of Dublin Dr. V. Ball was in intimate association, especially with the Royal Geological Society, of which he was the arduous Secretary for so many years.

In 1869 Dr. Ball married the eldest daughter of the late John Stewart Moore, of Moyarget, county Antrim. He leaves a family of four children. For some years Dr. Ball's health had been failing. About ten days previous to his death serious symptoms were manifested, and he passed away on the afternoon of Saturday, June 15th, at his residence, 28, Waterloo Road, Dublin.

PROFESSOR WILLIAM C. WILLIAMSON, LL.D., F.R.S.

BORN NOVEMBER 24TH, 1816. DIED JUNE 23RD, 1895.

BY the death of Professor W. C. Williamson, Palæobotany has lost one of its most earnest and energetic investigators and exponents, whose memoirs will long remain a record of persevering labour combined with remarkable genius and originality of thought.

William Crawford Williamson was born at Scarborough on November 24th, 1816. His father was for some time head gardener to the then Earl of Mulgrave, at Lyth Castle, near Whitby, where, having laboured indefatigably in exploring the geology and zoology of the coast of Yorkshire, and made a rich collection of its fossils and recent shells, he was, in 1828, appointed Curator of the well-known Museum of the Literary and Philosophical Society of Scarborough, amongst the collections of which much of his son's early youth was beneficially spent. Young Williamson was destined for the medical profession, but, in 1835, accepted the curatorship of the Museum of the Manchester Natural History Society. Whilst at Scarborough he contributed to the Geological Society of London the first of three memoirs on the "Vertical Distribution of the Organic Remains in the Strata of the Yorkshire Coast," and one to the Zoological Society of London on the "Birds of the Yorkshire Coast," as well as published a description of the well-known tumulus and its contents then recently opened on Gristhorpe Cliff. On reaching Manchester his attention was at once directed to the local geology, and soon resulted in the publication, in the Philosophical Magazine, of a memoir on the "Remarkable Limestones of Ardwick," which form the uppermost part of the Carboniferous strata in that neighbourhood.

In 1838 he resumed his medical studies, first in the Manchester Medical School, Pine Street, and afterwards in University College, London; and in January, 1841, he commenced as a medical practitioner in Manchester. Soon after that he began a series of investigations amongst the recent Foraminifera, the results of which were a succession of memoirs on their minute organization,

culminating, in 1848, in the publication, by the Ray Society, of his "Monograph on the Recent Foraminifera of Great Britain," and in a memoir on the minute organisms found in the marine mud of the Levant. This latter memoir contained the first announcement of the existence in some of the deeper seas of what is now known as the Foraminiferal Ooze. The study of some histological features of human bones and teeth led to an examination of the scales and bones of recent and fossil fishes. Two memoirs on these subjects were published in the Philosophical Transactions of the Royal Society, in which he announced his conclusion that the scales and dermal teeth of fishes were the homologues of the oral teeth of the mammalia, the latter being but the relics of the dermal system so extensively developed in fishes. The publication of these two memoirs led to his election as a F.R.S. in 1854. In 1851 the Owens College of Manchester began its career, when Mr. Williamson was elected its first Professor of Biology and Geology. As the institution expanded, this too-comprehensive chair was divided, and for many years past his academic labours had been confined to the Professorship of Botany. Circumstances then drew his attention to the Carboniferous plants of Lancashire and Yorkshire. The result of these later studies has been the publication, in the Philosophical Transactions, of seventeen memoirs "On the Organization of the Fossil Plants of the Coal-measures." On receiving the sixth of this series, the Royal Society recognized them by awarding him their Royal Medal. The Wollaston Gold Medal of the Geological Society was awarded to Dr. Williamson in 1890. Dr. Williamson was President, and subsequently senior Vice-President, of the Literary and Philosophical Society of Manchester. The University of Edinburgh conferred upon him the degree of LL.D. The Göttingen Academy of Sciences elected him one of its foreign members, and the Royal Society of Sweden elected him to the place left vacant by the death of Professor Asa Gray.

In the Royal Society's Catalogue of Scientific Papers from 1834 to 1873 is a list of 57 papers, principally upon microscopic organisms, Foraminifera, Radiolaria, etc.; on the microscopic structure of the scales, bones, and teeth of Fossil Fishes and Reptiles, and a most important series on the structure of coal-plants, *Calamites, Stigmaria, Sigillaria, Lepidodendron*, etc., with which his name will for ever remain honourably associated.

Prof. Williamson had retired from the Owens College, Manchester, for some time before his death, and had been residing at Clapham, where he had occupied himself with Dr. D. H. Scott, F.R.S., Hon. Keeper of the Jodrell Laboratory, Kew, in carrying on his researches in the microscopic structure of Fossil Plants, to illustrate which he had accumulated an immense and valuable series of microscopical sections and specimens.

Prof. Williamson having passed so many years of his life in Manchester had never become a Fellow of the Geological Society of London, although he was the recipient of its Wollaston Medal in 1890. He died at the age of 78 years, having been actively engaged up to a short time before his death.